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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF MICHIGAN.

Communicated by W. B. PILLSBURY.

V. THE RELATION OF THE FLUCTUATIONS OF JUDGMENTS IN THE ESTIMATION OF TIME INTERVALS TO VASO-MOTOR WAVES.

By H. C. STEVENS, A. B.

1. *Introductory.* What relation, if any, exists between the fluctuations of the judgment of time intervals and vaso-motor waves as shown by a finger plethysmograph? There were several suggestive facts that pointed to such a relationship. The plotted results of practice experiments in the estimation of intervals, made during the previous year, showed a fairly well defined tendency to regularity in the rise and fall of the curve corresponding to the large and small deviations of the judgment from the normal interval. As the length of these waves seemed to agree generally in length with the longer vaso-motor waves, a possible causal connection between the two series naturally suggested itself. There was also another source of suggestion. In an investigation into the relation between the fluctuations of visual attention and vaso-motor waves, made by Slaughter [*Amer. Jour. of Psych.*, Vol. XII, p. 313], it was found that the vaso-motor waves corresponded identically with the attention waves. One might, therefore, suppose that any activity dependent upon attention in so high degree as the estimation of intervals would be conditioned very largely by the vaso-motor waves that accompany it. A careful review of the literature of Time Sense has revealed but one reference to our particular problem. Stevens [On the Time Sense, Mind, Vol.

XI, p. 393] in referring to the curve of plotted judgments notes "distinctly longer or primary waves" which appeared in all of his results, and which seemed independent of the length of interval. The length of these primary waves was from 0.6 to 0.9 minutes. These rhythmical variations he thinks are due to rhythmical changes in the "standard carried about in the mind" of the subject. In explanation of the variations he says, p. 401, "That this is connected with the rhythmical changes in the nutritive condition of the cerebral centers, as produced by vaso-motor rhythmical constriction of the arterioles, it would be rash to deny or affirm, or, perhaps, even to suppose." In addition to the problem as already stated, there are two other points that have come in for consideration, namely, the determination of the course of the constant error and the question of the validity of Weber's Law. There are also several minor points that will be discussed as they present themselves.

2. *Method.* The method of Average Error, as given by Külpe was used in this study. There have been numerous objections to this method as applied to the estimation of time intervals, chief of which is that of Glass [Phil. Stud., IV, p. 423, Kritisches und experimentelles über den Zeitsinn], who was the first to use the method extensively after Vierordt. In criticising the method he gives three sources of error that tend to increase the constant error beyond its true value. "Diese Zeit setzt sich aus mehreren Elementen zusammen, nämlich erstens aus jener Zeit welche vergeht, um von dem Urtheil dass die Fehlzeit der Normalzeit gleich sei, zu dem Entschluss übergehen, den Gang des Uhrwerkes zu hemmen: dazu kommt zweitens die Zeit, welche nötig ist, den Bewegungimpuls auszulösen und bis zum Muskel fortzupflanzen, und drittens wird noch Zeit verbraucht um den Hebel zu verrüchen, wenn diese letztere Zeit auch nur einen sehr kleinen Bruchtheil einen Secunde beträgt. Da alle Fehlzeiten um T zu gross sind, so wird der constante Fehler in Wahrheit gleich c—T und also $\Delta = c - T$ sein." And in another place he says that, at times, it may become as large as $\frac{1}{10}$th of a second.

If this criticism were valid, the method of Average Error would have to be abandoned for the estimation of time intervals, at least; but that it is not true is clearly proved by the work of F. Martius [Weitere Untersuchungen zur Lehre von der Herzbewegung. Zeitschrift für klinische Medicin, XV, p. 536] in an investigation to demonstrate the ability of a person to register the beats of the heart, precisely at the moment of hearing them, by the "akustischer Markmirmethode." In an earlier paper [Vol. XIII, same Journal], Martius attempted to ascertain what point in a cardiogram corresponded to a given sound in the heart cycle. This he attained by recording,

with a tap of the finger, the point on the curve which corresponded to a given sound in the heart beat. In criticism of his work it was maintained by some that the movement of the finger could not be made simultaneously with the sound impression; but that there would be a difference in time between the hearing of the sound and the recording movement equal to the person's reaction time.

Martius's second paper is a reply to that objection. With the result, as he shows, that one has the ability to record a continuous series of sound impressions practically without error; the error varies between 0.04″ and 0.017″, and is very often zero. Furthermore, it must be remembered that the normal heart beat is a variable stimulus and, undoubtedly, accounts for much of the error. It seems certain, therefore, that the threefold error mentioned by Glass cannot be considered well founded.

The advantages of the reproduction method lie (1) in the immediate results of the estimation which it gives: obviating the error of indirect sensible discrimination. (2) It allows a more complete surrendering of the subject to the rhythm of the sound impressions. (3) And most important of all for our own problem is the continuous series of judgments which it permits without interruption. Such a series was absolutely essential for the problem in hand in order to afford a direct comparison of the series of plotted judgments and the parallel series of vaso-motor waves.

3. *Apparatus.* The apparatus in its essentials consisted of the following parts: (1) A large smoked drum, driven by a half kilowatt electric motor. (2) A tuning fork of 50 double vibrations per second. This was kept in vibration by a storage battery. (3) A Meumann Time Sense Instrument. (4) A clock-work kymograph for recording the normal interval, pneumogram and plethysmogram. (5) A telegraphic sounder. (6)[1] A finger plethysmograph. (7) A Fitz pneumograph.

There were three separate electrical circuits connecting the different parts of the apparatus, a description of which will make the workings of the apparatus, as a whole, more clear. (1) From the Time Sense Instrument to the telegraphic sounder, and thence by a shunt to a time marker which wrote upon the clock-work kymograph. This circuit was supplied by a battery of two bichromate cells. The Time Sense Instrument is supplied with adjustable contacts [two, only, were used in the experiments] to which one pole of the battery was attached. The circuit was completed by the instrument itself. The contacts, above referred to, are insulated so that the cir-

[1] Amer. Jour. of Physiol., Dec., 1899.

cuit is made and broken by the revolution of a metal rod. [For a detailed description of the Time Sense Instrument see Meumann, Phil. Stud., IX, pp. 270-274.] There are five pulleys on the instrument so that rates of rotation varying between fairly wide limits could easily be obtained. This, together with the different positions of the electrical contacts upon the graded circle of the instrument, admitted of all intervals of the lengths desired. The shunt in this circuit from the sounder to the time marker on the kymograph was put in for the purpose of having some constant point of reference for the plethysmograms and pneumograms. Thus, the relation of any given normal interval to the plethysmographic or pneumographic curves could easily be obtained. And conversely any point in the blood volume or respiratory records could readily be referred to its exact place in the series of normal clicks. Furthermore, as the rate of the drum on which the normal was reproduced was much faster than the kymograph on which the plethysmograms and pneumograms were recorded, the record of the normal interval on the latter drum made the work of plotting the judgments taken from the former drum easy and accurate. (2) A circuit, supplied by one bichromate cell, from the telegraphic sounder to a time marker which wrote upon the large drum. There was also a shunt circuit from the sounder to a key beside which the subject sat, and by which he reproduced the normal interval by a slight tap of the finger. From this it will be seen that one and the same signal served both to record the normal interval and the subject's judgment. The key was provided with a spring which immediately broke the circuit after the removal of the finger. (3) A circuit from the storage battery to the tuning fork and a shunt circuit in which was a resistance box, connected the tuning fork with the second time marker which wrote the time in fiftieths of a second on the large drum just above the curve of normal interval and reproductions.

4. *Practice and Proceedure.* Of the six persons used in this experiment but two were practiced in the estimation of intervals, although all were experienced in psychological work. With the shorter intervals no preliminary practice was considered necessary, but with intervals 0.92″ to 7.24″ a period of practice immediately preceded the regular experiments. The hour of experiment was 1 P. M. The general proceedure was as follows: The subject was comfortably seated beside a table so that his fingers hung just above the key by which his judgment was recorded. The middle finger of the left hand was thrust into the plethysmograph which was suspended from the ceiling by a cord. In the later experiments [Intervals 2.40″— 7.24″] a pneumograph was attached to the thorax of the subject and adjusted to write

upon the clock-work kymograph. The telegraphic sounder, the clicks of which limited the normal interval, stood upon the same table and behind the reagent. The clock-work kymograph stood near by, beside which an assistant sat who attended to it and to the writing of the plethysmograph and pneumograph. The large drum, Time Sense Instrument, tuning fork and motor were in an adjoining room, all connections with the other parts of the apparatus passing through the wall. There was considerable noise made by the motor; but as it was in the nature of a muffled roar of constant pitch and approximately constant intensity it formed a background as it were, for the two clicks of the normal, without, in any way, tending to "drown them out." Hence it was neglected. Two seconds before the motor was started the preparatory "ready" was called out by the assistant attending that part of the apparatus. The motor was then started, and also the clock-work kymograph in the other room. As the revolving rod of the Time Sense Instrument passes the two contacts the sounder emits two sharp clicks. The subject is to press the key at the moment he thinks that a time has elapsed equal to that between the two clicks of the sounder. Then follows the same sequence of the normal clicks and the tap on the key as before until the drum has exhausted itself. The subject is then allowed to rest while the drum is again prepared and a like series gone through with. The method of single reproduction, as above described, was not adopted until after interval 0.72″. Before that, a method of multiple reproduction was followed. The normal interval was given as already described; but the subject instead of dating his judgment from the second click of the normal, made *two* taps upon the key as the limits of the time interval. There was thus left a time interval between the last click of the normal interval and the first tap of the reproduction. With very short intervals when the contacts were close together there remained a considerable interval before the revolving rod completed its revolution. To take advantage of this the subject was told to make as many reproductions of the normal interval as he conveniently could. This, however, proved detrimental, as some of the subjects persisted in "hurrying" the last judgments, although cautioned against it. The number of reproductions, therefore, was limited to three, before this method was abandoned for that first described. The effect of both multiple and single reproductions upon the length of judgment will be discussed more fully under the head of "Constant Error." With the change in the number of reproductions the two contacts of the time sense instrument were placed 120° apart, and not changed during the remainder of the work. All changes in the length of interval were effected by means of the

belts and pulleys. The result of this is, that the two normal intervals recur at precisely the same point, with reference to the complete revolution, for any interval. They thus tend to fall into a definite rhythm, which is kept constant for all the intervals worked with.

5. *Review of Meumann and Shaw and Wrinch.* Nichols [*Amer. Jour. of Psych.*, Vol. III] has given a complete review of the literature of the subject up to eighteen ninety-one. Meumann has reviewed the later work of Münsterberg and Schumann. We will give, therefore, a brief statement of the results of work that still remains unnoticed.

Meumann: The work of E. Meumann makes a turning point in the nature of Time Sense investigation. Previous to him the general character of the work was very decidedly quantitative. That this is the case is very clearly shown by only a slight acquaintance with the literature of the subject. For example, Kollert and Estel formulated mathematical laws for the course of the constant error. Mach investigated only the question of the validity of Weber's Law. And Fechner's criticism of Estel is very largely a discussion of methods and an interpretation of symbols. In contradistinction to work of this kind Meumann's point of view is pre-eminently qualitative. The object of his investigation is to show the effect of the different intensities of the limiting stimuli on the apparent length of intervals and the deceptions of judgments due to different kinds of "fillings."

There are three articles that deal directly with the estimation of intervals. (1) Beiträge zur Psychologie des Zeitsinnes [Phil. Stud., VIII, p. 431], which is a critical review of the literature of the subject, with particular attention to Münsterberg and Schumann. (2) [Phil. Stud., IX, p. 264.] A continuation of the former article, though this is wholly experimental. (3) Beiträge zur Psych. des Zeitbewusstseins [Phil. Stud., XII, p. 128], which is an experimental investigation into the effect of different kinds of fillings upon the estimation of intervals.

As a general result of the first experimental work [Vol. IX], Meumann concludes that the estimation of intervals falls into three classes. (1) Very short intervals up to 0.5″, the judgment of which is very largely determined by the quality—intensity nature of the limiting stimuli. (2) Intervals of medium length — 0.5″ to 3.0″—which are estimated directly. The only basis of judgment is the interval of time itself. (3) Long intervals — 3.0″ and above — which are estimated indirectly by the mediation of some organic function. Meumann also points out the fact that, if, in a series of clicks of equal intensities, one more intense than the others, is sounded, the interval

preceding it is apparently shortened. And, in general, it may be said that an intense limiting stimulus shortens the length of the interval preceding it, while a weak stimulus apparently lengthens it.

The general results of the second experimental article [Vol. XII] on the effect of the kind of filling on the estimation of a time interval are recapitulated on p. 247 in the concluding remarks from which I quote.

(1) "Unsere Zeitschätzung ist in hohem Masse abhängig von der Art der Ausfüllung der Zeitstrecken."

(2) "Diese Abhängigkeit äussert sich in ganz verschiedener Weise bei kleinen, mittleren und grösseren Zeiten."

(3) "Werden zwei different ausgefüllte Zeitstrecken miteinander verglichen, von denen die erste eine reizerfüllte, die zweite eine "leere" reizbegrenzte ist, so erscheint bei kleinsten und kleinen Zeiten die reizerfülte Zeit grösser wie die reizbegrenzte Zeitstrecke; bei grossen Zeiten tritt das Umgekehrte ein, die reizerfüllte Zeit erscheint kleiner wie die reizbegrenzte. Zwischen diesen beiden Richtungen der constanten Fehlschätzung unter dem Einfluss differenter Ausfüllung lässt sich fast immer eine Indifferenzzone nachweisen, innerhalb deren die different ausgefüllten Zeitstrecken gleich oder annähernd gleich erscheinen, innerhalb deren also weder Ueber—noch Unterschätzung einer von beiden Zeitstrecken herrscht."

(4) The influence of the number of filling impressions is more strongly marked with small times. With both visual and auditory stimulation the quality of overestimation increases with the number of filling sensations so long as they are not carried to a point of fusion.

(5) These rules hold good in whatever sense department the limiting stimuli are.

Shaw and Wrinch. The latest work that has been done in Time Sense is "A Contribution to the Psychology of Time," by M. A. Shaw and F. S. Wrinch [Univ. of Toronto, Studies Psych., Series No. 2, 1898-1900]. The work is divided into five sections. (1) An Historical Sketch of the Problem. (2) Theory of Time Estimation. (3) Experiments on the Lapse of time between the normal and comparative intervals. (4) Weber's Law and Time Estimation. (5) Reproduction of complex rhythmically-arranged groups of interval. This work undoubtedly suffers from the fact that a preconceived theory of time estimation is allowed to influence the experiments. The theory is, that there is in consciousness a "unit of time" which serves as the basis of all time estimation. This unit is equivalent to "the interval that lies between the short and

long groups, which has been found to be the most accurately estimated interval. It differs in length more or less in different individuals, but in all cases the constant error and mean variation are less as we approximate to it. Here, neither the limiting impression, which influences the judgment of the shorter intervals, nor the originally functioned content which influences the judgment of longer intervals, modifies the estimation to any marked degree. This remarkable fact, in connection with the estimation of intervals, that a more or less definite interval is estimated most accurately, and the further fact that as we depart from it in either direction errors arise in judgment, lead us to believe it to be the basis of the estimation of intervals in all cases—a temporal unit."

The justification for the theory is that it satisfies the demands "fundamental in the human mind" for "single principles," and also the "scientific demand for explanation by as few principles as are consistent with the facts." p. 18.

The experimental basis of the theory rests (1) upon the statements of Külpe, Meumann and others, that there is an interval of medium length—0.5″ to 3.0″—which is judged immediately. (2) That intervals less than the indifference time are overestimated. "This error in estimation seems to be due to the tendency of the unit to complete itself, it being the individual basis of estimation." (3) With the estimation of intervals longer than the indifference interval, the organic functions, already mentioned, serve, at least, as helps to judgment. "The explanation is not, however, that the content is the basis of judgment, but that the mind estimates intervals longer than the unit in terms of the unit itself, and that the unit or the multiple of it, is here represented by some regularly recurring organic function." (p. 20.) Although one would expect some individual variations of the unit, the differences between the length of the indifference interval as given by Külpe 0.5″—3.0″ and those determined by their own experiments are very wide. Thus they say K's "unit of time" is "probably not much above 225σ″. W's is 150σ and S's 189σ. pp. 42-43.

The third section of the work, the experimental part, is to show the effect of a lapse of time between the normal and comparative intervals. In these experiments visual stimuli were used. The source of light was an incandescent lamp reflected by means of a mirror through holes bored in a band attached to the kymograph. The subject sat fifteen feet away, and received the stimuli through a telescope. As the drum revolved, intermittent flashes of light marked the interval. The intervals were reproduced by the subject "by means of a rigid steel wire, one end of which the observer held between his fingers and the other end passing round a pulley, was connected with

a pointer in contact with the paper." p. 126. The time was recorded upon the drum in hundredths of a second by means of a tuning fork. The average error, constant error and mean variation are worked out.

The results are quoted literally, p. 130. (1) The interval of half a second approximates to what has been called above the unit of time. . . . . Here, also, the effect of the pause is less than with intervals above it. The mean variation is the same for immediate [judgments] and "one minute," while the constant error is nearly the same for both. (2) With intervals above the unit of time the mean variation increases with the length of the pause. Sensibility decreases with intervals above the unit. (3) The constant error is practically the same throughout for "one minute" and "immediate." (4) After the lapse of half a minute the results are not regular: with the longest normal interval [153.66 hundredths of a second] the constant error is less than for the "immediate," but with the other normal intervals it is greater. The mean variation for a half minute is greater than for "immediate."

In the work upon Weber's Law and, incidentally, to show the effect of different intensities of the limiting stimuli, auditory stimulation was used. The normal intervals varied in length from 93σ to 9150σ. I give the results below in two columns. (1) The length of normal interval and (2) the fraction of Weber's Law, *i. e.*, the "mean variation of the percentages of mean variations" are given in average for six to ten normal intervals. I quote the results of several subjects using the most favorable result, as in some cases two results are given.

Subject.	Normal.	M v of %s M v's.
K	122– 257σ	0. 81%
	93– 187σ	0. 82%
	503–9050σ	0. 99%
W	101– 187σ	0. 72%
	131– 248σ	0. 67%
	512–9825σ	0.727%
S	107– 214σ	0. 62%
	500–8870σ	1. 50%
B	99– 254σ	1. 34%

From these results the authors conclude that it speaks "very conclusively for the validity of Weber's Law." It should be pointed out, however, that part of the results, at least, were made from auditory stimulation of varying intensities and the remainder with visual stimulation. It seems rather remarkable that the results agree as closely as they do, considering the very different experimental conditions under which they were de-

termined. But it seems very doubtful if Weber's Law can be said to hold for the estimation of intervals on the evidence of results that show such wide variations as those just quoted. They cannot be compared, for regularity, with those of Glass [Phil. Stud., IV, p. 453], who is very reserved in pronouncing the law "ideal."

6. *Experimental.* Under this head there are three main points that come in for consideration (1) and chiefly, the relation of the fluctuations in judgment to vaso-motor waves. (2) The course of the constant error. (3) The validity of Weber's Law. The six subjects who served throughout the entire investigation were Drs. Pillsbury [Py] and Slaughter [Sr] and Messrs. Bair [Br], Bryant [Bt], Pearl [Pl] and myself [Ss]. The intervals used varied from 0.18" to 7.24."

Our problem, as has already been stated, was to show what relation, if any, exists between the plotted curve of judgments and the vaso-motor waves as shown by a finger plethysmograph. There were two methods used to establish this relationship. In the first place the two curves were compared directly. That is, the plotted curve of judgments was superimposed upon the vaso-motor curve, and the variations in the former curve compared with the waves in the latter. The two curves were, of course, of exactly the same length; and any point in one had a corresponding point in the other. The second method may be called the method of averages. The estimation curve was superimposed as before, the beginning and end of each curve exactly coinciding. Then the crest and trough of any well defined vaso-motor wave was projected upon the curve of judgments. The judgments in the latter curve which coincided with points in the crest of the vaso-motor wave were then averaged. And in the same way the judgments coinciding with the trough of the vaso-motor wave were averaged, and the average judgment for the crest compared with the average judgment for the trough. This latter method proved, on the whole, to give better results than the first described. Although, in general, the large fluctuations of judgment seemed to agree with changes in the plethysmogram, still there rarely appeared an exact coincidence of both waves in the same direction, for more than four consecutive fluctuations. While the method of averages could be applied at any point in the curve at which there was a well defined vaso-motor wave.

The method of plotting the judgments was as follows: On the abscissæ were laid off the normal intervals, for any given series, as a unit. This unit was constant, of course, for any given normal interval. The normal interval, as recorded on the clock-work kymograph, ranged from two millimeters at the beginning of the experiments to ten at the end. So that in

order to ensure exact coincidence of the plotted curve and the plethysmogram, the millimeter paper was placed upon the plethysmographic record and the series or normals, as recorded on that drum, marked upon the millimeter paper. The judgments as they were obtained from the other drum in fiftieths of a second—multiplied by two to give hundredths—were numbered to correspond to the series of normals as recorded on the plethysmogram. So that there was no chance for error in the direct comparison of the two series. The judgments in hundredths of a second, one millimeter to one one-hundredth of a second, were laid off on the ordinate. To save space some number which was less than the smallest judgment was subtracted from all judgments of a given series before plotting. Thus only the absolute fluctuations appeared in any plotted curve. By this means curves to the number of thirty-seven were obtained which had parallel plethysmographic records with well defined vaso-motor waves in some part of their course.

As a result of the direct comparisons we have selected six curves which show coincidences of the vaso-motor wave and the fluctuations in judgment. The normal intervals from which they were derived, vary in length from 0.4″ to two seconds.

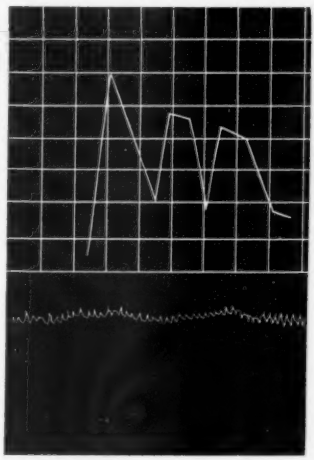


Fig. 1. Sr. subject. Interval N=0.66″. Curve reads from left to right.

The agreement of the three waves in each case of the two curves could hardly be bettered. The troughs of the curve of judgments exactly coincide with the troughs of the waves in the plethysmogram. Although the vaso-motor waves are not

extremely well marked, there is a very plain rise and fall nevertheless. On the other hand the curve of judgments—made up of ten judgments—is unusually regular. The specimens here given were taken both from intervals which were reproduced more than once and those only once. It would seem from a study of the plotted curves obtained, from both methods, that the number of reproductions was without effect on the fluctuations of judgment.

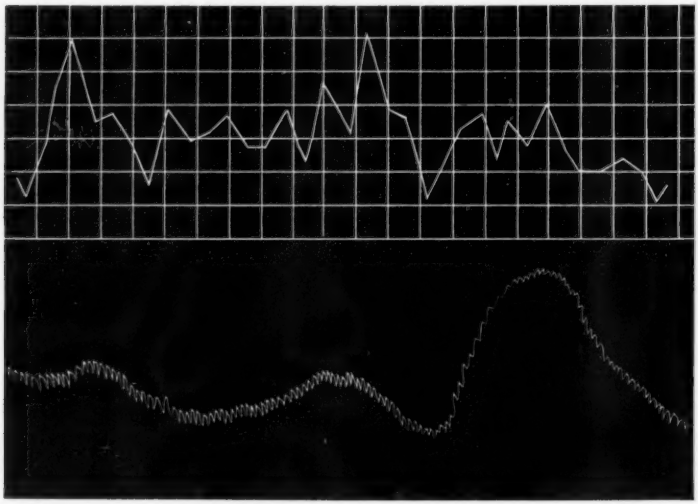


Fig. 2. Sr. subject. Interval N=0.77″. Curve reads from left to right.

The curves here exhibited were derived from the last interval in the estimation of which multiple reproductions were made. There are three conspicuous rises in the curve of judgments with corresponding crests of the vaso-motor waves. In particular, the first two largest deviations in the curve of judgments seem to have well marked counterparts in the plethysmogram. It is doubtful if the third very large rise in the plethysmogram is a Traube-Hering wave. However, it was not due to external causes, and besides it agrees fairly well with a general, though fluctuating, rise in the curve of judgments.

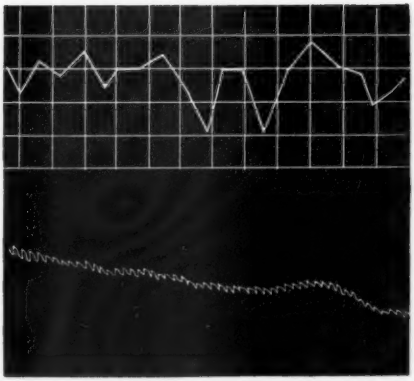


Fig. 3. Bt. subject. Interval 0.40″. Curve reads from left to right.

The interval from which these curves were derived was the first one of which single reproductions were made. The same deviation in the curve of judgments occur as in the others, although there is not much regularity. There are two well marked vaso-motor waves with corresponding rises in the curve of judgments. Between the two, however, is a short symmetrical rise and fall which agrees with a very slight rise in the plethysmogram between the two large vaso-motor waves before mentioned.

The plethysmogram in this case (see Fig. 4, p. 14,) is notable for the well marked and continuous series of vaso-motor waves. On the other hand, the variations in the curve of judgments are well marked but not very regular. In addition to the larger fluctuations there are many smaller ones which make it appear more or less broken. Of the eight vaso-motor waves that appear in this figure six have direct coincidences with the crests of fluctuations in the curve of judgments. On the other hand, the highest point in the estimation curve corresponds to the lowest of the plethysmogram. The remaining vaso-motor wave has no direct agreement with a rise in the judgments. This curve is also notable for the fact that it admits of direct comparisons for a longer period of time than any of the other curves.

As is shown in Fig. 5, p. 15, the fluctuations in the estimation curve are much larger than those previously exhibited. Although the agreement between the two curves can hardly be said to be exact, yet there is some coincidence, and what is more, the curve shows as many others did, though no direct comparison could be made, that where the changes in the ple-

Fig. 4. Bt. subject. Interval 0.92″. Curve reads from left to right.

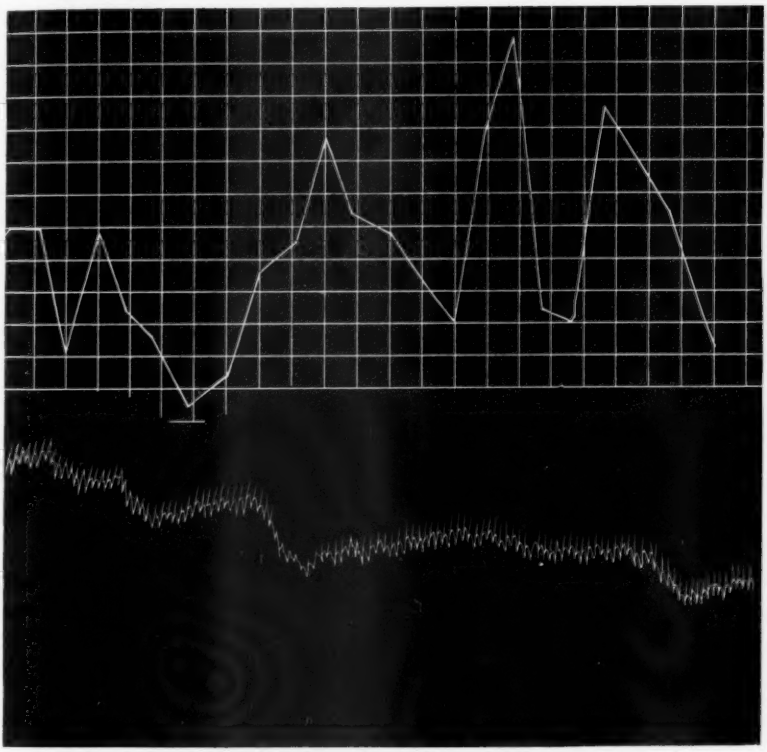


Fig. 5. Pl. subject. Interval 0.92″. Curve reads from left to right.

thysmogram are largest, that part of the estimation curve corresponding to it shows greatest variations. There is another point which this curve exhibits in common with those of the large intervals, that, as the intervals grow longer, the deviations of judgment from the normal interval grow larger.

There are only two vaso-motor waves in Fig. 6, p. 16, that can be said to correspond exactly to large deviations in the curve of judgments. Although, as was observed in the preceding curve, the fluctuations in estimation are largest with corresponding changes in the curve of blood volume.

As was said earlier in this article, the method of direct comparison does not yield as conclusive evidence as does the method of averages. And in either case, it must not be supposed that

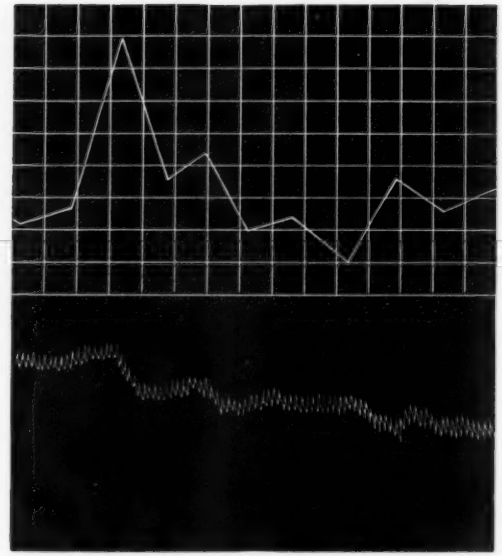


Fig. 6. Pl. subject. Interval [approximately] 2.00″. Curve reads from left to right.

the vaso-motor changes are the only factors at work in the estimation of intervals. If this were true one would expect invariable coincidences of the two series. As it is, there are a great mass of factors more or less obscure which must enter into the judgment of a length of time, and in view of this the absolute functioning of any one factor—as the vaso-motor changes in our case—is not to be expected. As other factors which would tend to influence the judgment may be mentioned the strain sensations of Münsterberg which are undoubtedly present; the emotions, surprise and expectation, of Schumann; the quality and intensity of the limiting stimuli; the objective and subjective rhythms of sound impressions. Then there are distractions of various sorts, and such factors as fatigue, practice, individual differences and varying experimental conditions. All of which tend to obscure any single factor.

In Table A are given the results of the method of averages. In the first column is given the number of the experiment. The second column gives the initials of the reagent; the third the length of the normal interval; the fourth the average of the judgments that correspond to the crest of the vaso-motor wave; the fifth the number of judgments that enter into a correspond-

TABLE A.

No.	Reagent.	N.	Crest.	n.	Trough.	n.	Difference.
1	Pl.	0.18	.199	24	.177	7	+.022
2	Ss.	0.48	.477	8	.482	4	—.005
3	Pl.	0.48	.523	14	.508	7	+.15
4	Pl.	0.48	.487	8	.465	11	+.22
5	Py.	0.66	.771	12	.700	7	+.71
6	Sr.	0.66	.822	7	.678	7	+.144
7	Pl.	0.66	.722	13	.657	8	+.65
8	Sr.	0.72	.896	9	.790	5	+.106
9	B.	0.40	.360	3	.280	2	+.800
10	B.	0.40	.430	2	.330	4	+.100
11	Ss.	0.40	.445	2	.350	2	+.950
12	Py.	0.40	.436	14	.384	11	+.520
13	Pl.	0.40	.395	13	.363	11	+.320
14	Py.	0.70	.730	9	.630	14	+.100
15	B.	0.92	.861	11	.828	7	+.330
16	Sr.	1.32	1.26	9	1.16	6	+.100
17	Ss.	1.32	1.26	8	1.13	10	+.130
18	Sr.	2.40	2.23	2	2.00	1	+.230
19	Py.	2.40	2.73	6	1.91	6	+.640
20	Pl.	2.40	2.36	.4	1.91	6	+.450
21	Py.	2.40	3.46	3	2.32	4	+1.140

ing average; the sixth the average judgments that occur at the trough of a vaso-motor wave; the seventh the number of judgments that enter into that average; the eighth the difference between the average crest and the average trough.

The last column in this table is the one for which it was designed. The difference between the average crest and trough shows only one case in which the latter exceeds the former, and that difference is but 0.005″ in favor of the trough. And on the other hand, the increase of the crest over the trough varies from 0.022″, the smallest to more than one second, the largest difference. While on the average the difference is very decided in favor of the crest.

This table shows much more conclusively the relation of the vaso-motor wave to the curve of estimation than is afforded by a direct comparison of the two curves. And it seems not unwarranted to conclude that the vaso-motor waves are, at least, one of the determining factors in the variations of the judgment in the estimation of time intervals. To attempt to ascribe all the phenomena observed in the estimation of intervals, to them would be absurd, and it is too much to demand an unexceptional coincidence in every case; yet that there is in at least 50% of cases an agreement between the two series seems from our experiments to be certain.

It will probably be noticed in Table A that the largest interval is 2.4 secs., although there were three other intervals

longer than this. The reason for that is that the plethysmograms showed hardly any vaso-motor waves. This was due, in all probability, to the unusually cold weather during which the last experiments were made, and also to the fact that the building was not heated. It is barely possible that their absence is due to the inhibitory effects of the longer intervals.

The difficulty that was experienced by most subjects with the longer intervals, and in particular, interval 5.45″ showed itself very plainly in the plotted curves. With the smaller intervals the amount of variation in either direction from normal the interval was comparatively small; but with longer intervals the uncertainty of judgment showed itself by the extremely large variations on both sides of the norm. In the last intervals this often amounted to two or three seconds.

The Constant Error. With reference to the constant error our own experiments as a whole divide themselves into two groups, all dependent upon the number of reproductions of the normal interval. In the first group [Intervals 0.18″-0.72″] there were, at first, five or six, and finally two reproductions made after each normal. But as this procedure proved detrimental by causing some of the subjects to hasten their judgments it was abandoned for the single reproduction method. In the second group [Intervals 0.40″-7.24″], therefore, but one reproduction was made for each presentation of the normal.

The effect of the number of reproductions upon the size of the constant error has received too little investigation, as yet, to speak with much certainty about it. As Nichols says, "New experiments seem needed for the tripartite relations between the sign of the Constant Error, the number or length of time the norm is given as a sample, and the number of reproduced judgments." Stevens [Mind, XI, p. 393], however, used the method of multiple reproduction, and has published the average judgment for each five seconds after the normal to the end of the series. The result is a very decided diminution of the size of judgment for each successive five seconds, as the series taken from Table II, p. 395, will show:

N=1.44 (1) .960 (2) .830 (3) .740 (4) .710 (5) .677 (6) .610 (7) .590 (8) .480 (9) .360. This result is common to all of the tables as published. Nichols also used the multiple reproduction method, but has published only the first judgments after the normal. He says, however, that for one subject, L, the judgments are "unusually short throughout the experiments, and, in the curves, show themselves growing rapidly shorter and shorter to the end of the drum," while the "judgments of H. are unusually long throughout all his trials, and his curves go rapidly up throughout each drum." In our own experiments with the multiple reproduction there were, also, oppo-

site tendencies to be observed in different subjects. Thus Br. and Bt. showed a constant decrease with each reproduction, which very probably accounts for the small indifference time of Br. [0.46″] and the irregularities in Bt's constant error [Table IV]. While, on the other hand, the remaining subjects showed a constant enlargement of each successive judgment. Although the number of reproductions was, probably, too small to say finally what the effect would be.

From our own experiments the effect of the single reproduction may be inferred with more certainty. With but two exceptions all subjects show a positive error at 0.46″. One of the exceptions, Bt., gives a positive error at 0.58″. These intervals were estimated by multiple reproductions. On the other hand, all subjects but Py. [c=+.018] show a marked underestimation of the first interval [0.40″] in the second group. We may conclude, therefore, that the effect of the single reproduction is a decided lowering of the first indifference point.

The results of each reagent are given in a separate table. There are six columns given in each table. (1) The number of the interval in order of size. (2) The normal interval in hundredths of a second. (3) The constant error in hundredths of a second with the positive or negative sign prefixed to indicate an over or underestimation, respectively. (4) The average crude error. (5) The pure variable error. (6) The number of experiments made on each interval.

TABLE I. Py.

No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.18	+.023	1.860	.1035	506
2	0.34	—.040	2.815	.2843	400
3	0.46	+.026	4.091	.0889	333
4	0.58	+.100	4.565	.07871	119
5	0.66	+.060	7.0671	.10705	117
6	0.72	+.027	4.509	.06262	173

No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.40	+.018	3.488	.08708	261
2	0.70	—.028	4.441	.06605	241
3	0.92	—.017	7.516	.0816	172
4	1.32	+.18	14.434	.1099	139
5	2.40	+.235	31.1292	.1296	61
6	3.70	+.65	53.261	.14391	65
7	5.45	+.573	64.302	.11673	40
8	7.24	+.443	93.267	.12882	32

The constant error, as shown by Py's results, is, in general, a typical instance of its course as exhibited in our experiments.

Yet there are some peculiarities that are not common to the other subjects. One striking exception is the negative constant error at 0.34″ in the first group. The reason for this, probably, lies in the fact that interval 0.34″ was the first one used in the beginning of the investigation. Hence, the lack of practice may account for it. Nichols also points out that any "slight nervousness or excitement of the subject shortens the judgment." This, too, may have had something to do with it, The change of sign of the constant error at interval 1.32″ in the second group is also somewhat anomalous. But as the overestimation after this is constant the cause will have to be ascribed to some individual peculiarity.

TABLE II. Br.

No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.18	+ .004	1.5803	.08834	371
2	0.34	+ .025	1.987	.05840	422
3	0.46	— .045	2.730	.05932	509
4	0.58	— .011	3.524	.05990	129
5	0.66	— .049	4.2306	.06409	109
6	0.72	—1.02	3.893	.05406	132

No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.40	—.035	4.079	.1019	244
2	0.70	—.068	4.792	.06827	227
3	0.92	—.073	4.823	.05237	183
4	1.32	—.013	12.093	.0916	139
5	2.40	—.117	17.350	.07229	40
6	3.70	+.02	47.999	.12973	44
7	5.45	+.160	254.570	.46706	35
8	7.24	—.30	63.030	.08705	34

The small indifference time of Br. [0.46″] in the first group is probably due, as I have already said, to the constant tendency to decrease the size of each successive reproduction. Yet it may be wholly normal as the indifference time as given by Külpe varies between 0.5″ and 3.0″. The sign of the constant error in the second group changes somewhere between 2.4 secs. and 3.70″. It also, again, changes signs at 7.24 secs. What this last change is due to I am at loss to know.

The points to be noted in the course of the constant error in Table III, p. 21, are the negative sign of the error at 0.72″ in the first group and the small underestimation of the first interval in the second group. There is also another indifference point between 2.4″ and 3.70″. From there on the overestimation is very large.

TABLE III. Ss.

No.	N.	c.	$\triangle m$	$\frac{\triangle m}{N}$	n.
1	0.18	+ .039	1.622	.09011	371
2	0.34	+ .027	1.769	.0502	126
3	0.46	+ .011	3.312	.07199	184
4	0.58	+ .017	5.1965	.08959	98
5	0.66	+ .232	12.55	.18999	129
6	0.72	—1.01	6.660	.09249	134
No.	N.	c.	$\triangle m$	$\frac{\triangle m}{N}$	n.
1	0.40	— .002	3.431	.08617	114
2	0.70	— .087	5.086	.07265	269
3	0.92	— .042	4.375	.0475	172
4	1.32	— .152	8.3852	.06396	91
5	2.40	— .267	18.942	.0812	32
6	3.70	+ .886	68.536	.18532	60
7	5.45	+1.5	131.08	.23984	36
8	7.24	+1.02	111.906	.15456	33

TABLE IV. Bt.

No.	N.	c.	$\triangle m$	$\frac{\triangle m}{N}$	n.
1	0.18	+.008	1.2022	.06678	536
2	0.34	+.040	2.180	.06411	22
3	0.46	—.038	3.100	.0674	254
4	0.58	+.015	3.6285	.06255	123
5	0.66	—.053	3.1134	.0471	132
6	0.72	—.066	4.1562	.05272	110
No.	N.	c.	$\triangle m$	$\frac{\triangle m}{N}$	n.
1	0.40	— .011	6.009	.1502	237
2	0.70	— .046	3.959	.0597	211
3	0.92	— .060	4.4365	.04822	177
4	2.40	— .080	17.452	.0745	73
5	3.70	+ .020	47.13	.1135	45
6	5.45	+1.23	85.554	.14461	45
7	7.24	+ .43	108.378	.14969	31

The constant error as derived from Bt's results shows considerable variation in the first group; the first change of sign at 0.46″ and also the large negative error at 0.66″ are in all probability due to the multiple reproduction. Yet the positive error at the interval [0.58″] between these two seems to contradict this, as it was obtained by the same method. The constant error in the second group agrees completely with the results of the other reagents, showing the underestimation from

0.40″ up to 2.40″ and an overestimation from 3.70″ on to 7.24″.

TABLE V. Pl.

No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.18	+ .021	1.3355	.07419	346
2	0.34	+ .055	2.0537	.06035	186
3	0.46	+ .034	2.7260	.05925	199
4	0.58	+ .065	3.595	.06197	121
5	0.66	+ .037	3.7449	.05381	134
6	0.72	+2.03	8.0132	.10828	79
No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.40	—.022	2.620	.0655	247
2	0.70	—.1005	6.506	.09291	230
3	1.32	—.200	12.546	.09650	64
4	2.40	—.255	24.329	.10136	70
5	3.70	+.59	54.539	.1475	43
6	5.45	+.48	46.075	.08453	42
7	7.24	+.60	46.969	.06483	24

The constant error in Pl's results in the first group show very decided overestimation in every case, and particularly at 0.72″. This is probably due to the fact that Pl. was one of the subjects who showed successively larger reproductions after the normal. The second group agrees in general with all of the others, showing an underestimation of the interval up to 3.70″, and from there on a very decided overestimation.

TABLE VI. Sr.

No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.18	+.053	1.599	.08832	274
2	0.46	+.063	3.7806	.08218	176
3	0.58	+.078	4.7255	.08147	126
4	0.66	+.117	4.9906	.06861	125
5	0.72	+.055	6.1345	.08289	126
No.	N.	c.	Δm	$\frac{\Delta m}{N}$	n.
1	0.40	—.0325	4.043	.1009	246
2	0.70	—.204	6.629	.09445	233
3	2.40	—.19	18.038	.07515	100
4	5.45	—.17	50.034	.09273	43
5	7.24	+.40	58.453	.08073	33
6	....	.....	.....	.....	...
7	....	.....	.....	.....	...

Sr. like Pl. was one of the reagents who gave successively

larger reproductions, and the large overestimation of all of the intervals in the first group is very probably due to that fact. In the second group the change of sign of the constant error does not occur until 7.24″. This may have its origin in the lack of reacting to some of the longer intervals, as no results were obtained from him on intervals 0.92″, 1.32″ and 3.70″. So that a considerably longer time elapsed between his experiment days and those of the other subjects.

The general course of the constant error shows an overestimation [in the first group] of intervals up to 0.72 seconds. Then an underestimation [in the second group] from 0.4″ to 2.4″, from which interval on the sign of the constant error is reversed, with an increasingly large overestimation. These results agree very closely with those obtained by Mehner [Phil. Stud. XI, p. 546] by the method of Average Error. The following table gives the normal intervals and the corresponding constant errors as obtained by him. p. 594.

N.	C.
0.70	+.09
0.80	—.025
1.00	—.068
1.50	—.1117
2.10	—.035
3.50	+.01
5.00	+.0375
6.00	+.94

With the exception of a disparity between the sign of the constant error for 0.70″ the two results agree very harmoniously. Mehner's second indifference point lies between 2.10″ and 3.50″, and our own between 2.40″ and 3.7″. Just at what point the sign changes would be impossible to conclude from either set of experiments. But, taken together, they would show that it lies between 2.4″ and 3.5″.

Weber's Law. The validity of Weber's Law for the estimation of intervals of time is still a moot point. There is, perhaps, an equal division of opinion among the many investigators who have discussed this problem. The results of Glass [Phil. Stud., IV, p. 453] as given in Table III show a greater constancy of the fraction of Weber's Law than any that I have seen. Of the results obtained from thirty-four intervals, in twenty-nine cases the first two figures are four hundredths, and in the remaining five cases five hundredths. From these results Glass concludes that Weber's law in the Province of Time Sense is to be considered an Ideal Law as Mariotte's Law is in Physics.

From our own results it would be very difficult to conclude

in favor of the law. For, not only are there many irregularities, but also there seems to be an increase in the value of $\frac{\Delta m}{N}$ as the larger intervals are reached. Manifestly, if such is the case, one could not maintain that the law held. There is, however, a peculiarity that should be pointed out. Namely, that each subject seems to have an individual value of $\frac{\Delta m}{N}$ from which variation arise in either direction. In the case of Br. it is in the neighborhood of .0500 or .0600, while with Sr. it is most nearly constant at .0800. And it is in Sr's results, in general, that there seems to be an approximate constancy of the value of the average pure error.

Respiration as an Aid to Estimation. According to Münsterberg's theory of time estimation, the strain of muscles during respiration is made the basis of time estimation. We have endeavored to ascertain by the introspection of reagents just what part, if any, the strain of respiration does play. A pneumograph tracing was taken in the experiments from interval 2.40″ to 7.24″. The tambour was adjusted to write just over the series of normal intervals as obtained on the kymograph. So that by projecting the normal interval upon the respiration curve the phase of breathing in which the subject was when the limiting stimuli were given could easily be determined. And, likewise, the phase of breathing in which the judgment was made could be determined.

It may be stated generally from the introspection of the reagents that there was considerable tendency, and, in many cases, actual use of the respiration as a measure of the longer intervals. Thus, Br., Interval 3.70,″ says he "found himself estimating the interval by respiration, and could not keep his attention off from it." He expired on receiving each of the limiting impressions. Also with the interval next higher, 5.45,″ Br. reports that he intended to make his estimate by breathing. With the last interval, 7.24″, the same subject noticed a similar effect. He tried hard to keep attention off his breathing, but could not avoid reacting at expiration. Bt. reports that for interval 5.45″ he estimated entirely by respiration. But for interval 3.70″ and 7.24″ he did not use it, although in both of these intervals he used other means. With intervals 3.70″ and 7.24″ Py. reported the presence of strain sensations. With the shorter interval he noticed a tendency to estimate in terms of strain of respiration. With interval 5.45″ he observed that he "could not estimate definitely in strain sensation; but tended to do so," he "noticed that his judgment fell in the same phase of respiration as the normals." For interval 3.70″ Pl. reported that "during the first series all attention was given to estima-

ting the interval by relating it to breathing. There was a very good relation between the normal interval and a certain number of inspirations and expirations ("two inspirations and one expiration equal to the normal interval). Then the estimate of the interval was made from this relation." Pl. was the only reagent who had any tendency to estimate the interval by counting. In one case, interval 3.70″, he filled the interval with a rhythmical repetition of "one" "two" "instead of a consecutive count during the whole interval." Again, with interval 7.24″, he says that "the judgments were made by counting consecutively up to nineteen or twenty." Sr., interval 5.45″, is peculiar in being the only subject to report the presence of the strain of abdominal muscles in consciousness. He says that the strain sensations were most conspicuous at the end of the normal interval and at the moment of his reaction.

By projecting the point, indicated on the clock-work kymograph by the first click of the normal interval, upon the respiratory curve, the relation between the two could be plainly brought out. And almost universally for any given series, the relation between the two remained constant throughout. That is, for example, if the first click of the normal interval enters at the crest of the respiration curve, between the expiration and inspiration, the first click of the next normal interval will enter at precisely the same point with perhaps five or six complete phases intervening. If the point of entrance is shifted then there is a corresponding shift of the point of entrance of the next normal. These relations held generally for all subjects during the intervals in which the pneumograph was used.

The point in the respiratory curve at which the judgment was made, has been determined for three subjects only, in one interval [3.7″]. Time would not allow longer investigation. As has been said, the normal interval was obtained upon the kymograph, in addition to the tracing upon the revolving drum, and of course the time of the interval was known. Hence, to obtain the time of rotation of the kymograph, the distance in millimeters, between two normal clicks was divided into the lengths of the interval, which gave the length of time per millimeter. Then, dividing the judgment in time, by this quotient, gave the length of judgment in millimeters which could easily be measured from the second click of the normal interval and the point in which the judgment was made, accurately determined. This point, projected upon the respiratory curve gave the phase of breathing in which the judgment was made.

This point, as determined in the three cases alluded to, shows an approximate constancy for any given series. For example, in Pl's case, the judgment is made in two series at

the trough of the respiration curve, between the phases of inspiration and expiration. This holds, generally, throughout; but if the point is changed it remains constant for the remainder of the series. Likewise, with Py. and Br. The judgment in Py's case falls at the crest of the respiratory curve between the phases of expiration and inspiration. But, in general, it remains constant throughout any one series. Br's judgment is made at the beginning of the inspiratory phase. And like the others, it is generally constant.

It cannot be concluded from these three cases that the relations observed here hold good for other subjects or for other intervals.

Summary of Results.

1. The vaso-motor wave coincides, in at least 50% of the cases, with the fluctuation in the judgment of an interval of time.

2. For intervals above 3.7 secs. the strain of respiration may be used as an aid to estimation.

3. The method of single reproduction tends to lower the indifference point.

4. Intervals below 0.72″ [0.40″] are overestimated. From that interval to 2.4 secs, the intervals are underestimated. From 3.7″ to 7.24 secs. the intervals are overestimated.

5. Weber's Law does not hold for the estimation of time intervals.

6. There is no special Time Sense in consciousness; but our judgment of time is mediate, depending upon organic processes, of which the changes in blood volume is one of the more important.

Conclusions.

It has been stated in the summary that the vaso-motor wave in, at least, 50% of the cases, coincides with the fluctuations in judgment of a time interval. While this is not a predominating per cent. of all cases, still it may be taken as a positive answer to the problem as stated at the outset. It should be remarked here that the best coincidences and, in fact, all of those exhibited in this paper, occurred on intervals of time from 0.4″ to 2 seconds in length. This is borne out by many of the curves not published. This would seem to indicate some sort of connection between the indifference time or "unit of time" and the vaso-motor waves. But just what the relation is can not be determined from these experiments. The general length of the vaso-motor wave, as shown in our results, varied from five to twenty seconds. The general length of the fluctuation in judgment likewise varied from six to thirty-five seconds. On the whole there seemed to be a gradual increase in the length

of both vaso-motor wave and fluctuation of judgment as the length of the interval was increased. But, in any case, the two kinds of variations stay fairly well together.

In attempting to determine the part played by the vaso-motor wave and respiration, in the estimation of intervals, there seems to be an obvious line of distinction between the two processes. As has been said, the best and most frequent coincidences of vaso-motor curve and curve of judgments occurred at intervals of 0.4″ to 2.0″ in length. On the other hand, it was not until interval 3.7″ had been reached that subjects began to report any tendency to use respiration as a measure of the interval. Bearing these two facts in mind, it would seem that each function plays a more influential part according to the length of the interval estimated. For intervals of medium length [0.4″—2.0″] it would appear that the vaso-motor changes, of all factors, predominated. While for longer intervals [3.7″—7.24″] the respiration is, apparently, of greater influence. Yet it must not be inferred from this that the vaso-motor changes and respiration are the only factors involved in the estimation of intervals. We can only say that they were most conspicuous in our experiments. As to muscle strains alone, apart from respiration our experiments offer but little evidence. In fact they were reported by but one subject, and that for only one interval. So that their function, if any they have, must remain undecided by these experiments.[1]

[1] I wish, here, to thank the gentlemen who kindly gave their time to serve as subjects in this investigation. Particularly, Mr. J. H. Bair, who acted as assistant. It was due to his careful work that the plethysmograph yielded the good results that it did.

A CONTRIBUTION TO THE PSYCHOLOGY OF RHYTHM.

By CHARLES H. SEARS, Fellow in Psychology, Clark University.

The paper here presented is a report of an experimental study of the time values given by competent players to the notes of several simple selections of music. When the musician begins his studies certain statements are made to him with regard to the relative values of notes. He is told that a half note should be given half the time of a whole note, a quarter note half the time of a half note, an eighth note half as much time as a quarter, and so on, and that a dot placed after a note adds one-half to its length. It is implied that all notes of the same kind should receive equal amounts of time unless a change of tempo is indicated, that a triplet should divide into three equal parts the time usually given to two like notes, that, except for purposes of expression, all measures are of the same length, etc. Toward fulfilling these requirements he strives with the metronome as an assistant. How far the trained musician accomplishes what the notes set before him indicate and what he sets out to do is an interesting question not only to the psychologist, but also to the musician. To answer this question for a few simple cases by direct measurement of the lengths of the notes played has been the aim of this study. The problem was suggested by Meumann, but so far as I know, no investigator has offered a contribution of facts gathered directly from the records of performers, although some work has been done in closely related fields as will presently be shown.

The work was undertaken at the suggestion of Dr. E. C. Sanford, to whom the writer most cordially acknowledges his obligation for help rendered throughout its progress.

RÉSUMÉ OF LITERATURE.

Because of the publication of Meumann's able working over of the whole subject of rhythm[1] it is natural to begin with him. We shall of course confine ourselves here to his discussion of rhythm in music, which he considers from the standpoints of the hearer, the player, and the composer.

In musical rhythm there appear not only the elements that

[1] Untersuchungen zur Psychologie und Æsthetik des Rhythmus, *Philosophische Studien*, Bd. X, S. 300.

are found in rhythmizing of uniform sounds and other simple rhythmical material, but also a number of new elements due to the special rhythmical material or rhythmizomenon in question. Working upon these the universal pleasure in tones and their combinations has brought about the peculiarly rich artistic development of musical rhythms. In a corresponding way the intellectual factors on which depend the perception of melody and harmony and the comparison of the different parts of musical compositions, as well as the play of association, memory, and fancy, are all more complicated and intense than with simple sounds. The same is true, of course, of the feelings and emotions excited. It is of interest to note, however, that in the perception of a rhythmical series of simple sounds the rhythm holds the first place, while in music it yields at least a co-ordinate place to the other distinctively musical factors.

But quite aside from this, the tones themselves do not behave like simple sounds. A low tone, for example, appears less intense than a high tone of the same objective intensity; the sudden breaking off of a quickly played tone series gives to the last tone a sort of accent, to be explained on the ground of sound summation. Tones are also possessed of real quantity, *i. e.*, they can be continued for a longer or shorter time, a thing that is not possible with simple noises; and with this belong also all the rhythmical effects arising from legato and staccato playing.

In treating rhythm from the standpoint of the player Meumann discusses the question of how the musician is able to produce the exact fractions of time intervals required by the notes on the written sheet. Experiments made upon subjects skilled in piano playing to determine how accurately intervals equal to certain fractional parts of given intervals could be produced when there were no artificial means of assistance showed that even good musicians were liable to considerable errors. A second test was made in relation to the observance of tempo. In this a good piano player was required to beat a rhythm first in accompaniment to a given standard rhythm and then alone. The intervals between the single strokes of the beating were 0.4 and 0.3 seconds. After the observer had beaten the rhythm thirty to forty times the standard rhythm was discontinued, while the subject kept on. As soon as the standard rhythm ceased the beating of the subject began to change. The tempo was first retarded, then the relative times of the intervals were altered, and gradually a change followed in the rhythm itself. These tests make it probable that the musician must have at his command in his ordinary playing some means or other of assistance. This means of assistance Meumann finds in a cer-

tain motor appreciation of duration; and the high degree of accuracy with which a musician can produce a required fraction of a standard interval when it is part of a musical score must be ascribed largely to this motor appreciation, *i. e.*, to the facility with which a rhythmic movement becomes automatic. In playing with both hands another means of control is possibly furnished by the aid one hand may give to the other. For example, when the right hand plays a complicated passage, the left supported by its automatism can preserve the original rhythm, marking for the right hand the essential points of each measure. Counting introduces still another motor procedure, which becoming quickly automatic acts as a steadying influence. The movements of the director have a similar result. The effect of all this automatism is to unburden the attention so that it may concentrate itself upon the musical purport of the tones.

To Binet and Courtier[1] belongs the credit of having recorded by the graphic method the fingering of pianists with indications of the force and time relations of the strokes and of suggesting a number of problems that can be solved in this way. Their results may be summarized in part as follows: The fingers of pianists are unequal in strength and lack independence. This follows naturally from the anatomical structure of the hand. In the execution of trills played with the index and middle fingers the irregularity of the movements, as shown by tracings, is marked. However, a high degree of regularity results from practice. In playing the piano notes are frequently slurred or tied. These effects may arise from a variety of conditions, three of which are: "1. In the movements of the last fingers, especially of the third and fourth. 2. In the state of fatigue; the slurring or tying of notes is a rest for the sluggish or fatigued hand. 3. In movements of great rapidity." The movements of the thumb vary in facility of execution; those coming after the third and fourth fingers are more difficult to perform than those that follow the first and second. The notes preceding the thumb movements have a diminished intensity. In slow playing the notes are rarely tied or slurred, and both the notes and intervals are regular. But as the rate of movement increases the irregularities become more and more apparent. A renowned musician in playing five successive notes was found to retard the interval between any two notes 0.01 of a second. In ten successive trials it was observed that the value of the intervals always increased as they advanced up the scale, the average increase varying from 0.01 to 0.015

[1] Recherches Graphiques sur la Musique, L'Année Psychologique, 1895, p. 201.

of a second. In relation to the accentuation of single notes these investigators found that a tendency exists—" 1. to separate the accented note from the preceding note, 2. to tie or slur the accented note to the following note, 3. to increase the length of the note accented as if this increase were equivalent to an increase in intensity, 4. to increase, especially in rapid playing, the intensity of the notes which follow the note accented."

Ebhardt has made two contributions to the psychology of rhythm and tempo[1]—one on the influence of accent on the relations in musical rhythm and the other on the influence of accompaniment on the tempo. In order to discover the effect of rhythmic accent on temporal relations he first conducted a course of free tapping experiments consisting of both accented and unaccented series of sounds. He found that absence of accent was attended by a want of uniformity in the time relations of the members, but that there was no constant increase or decrease of the times in the course of the series. His results show that the greatest uniformity of movement lies approximately within that range of rates within which Vierodt[2] found the greatest exactness of judgments, that of 0.4-0.7 seconds. In the case of series with accent, experiments were made upon the different forms of bipartite and of tripartite rhythms. In all of these forms the variations of the members from one another appeared far greater than in the unaccented series, *i. e.*, the errors were the largest in the accented series. The conclusions drawn from these experiments are that a rhythmic accent exerts a disturbing influence on the keeping of time, that it is a source of constant error, and that in the great majority of cases the accent gives rise to a lengthening of the time of the accented member. There is much evidence for the opinion that accent causes not a shortening of the preceding but a lengthening of the subsequent member. In two forms of the tripartite rhythm—the one in which the second member and the one in which the last member receives the accent—when the repetitions were somewhat lengthy, the accented member showed a tendency to change gradually its order in the group. In the production of tripartite rhythms the groups were separated into unities by means of an inserted pause. This was the most easily done and the most distinctly marked in rapid tapping. As the movements became slower this separation of the groups gradually faded out.

The investigations on free tapping were followed by experi-

[1] Zwei Beiträge zur Psychologie des Rhythmus und des Tempo, *Zeitschrift für Psychologie und Physiologie der Sinnesorgane*, Bd. XVIII, p. 99.

[2] Untersuchungen über den Zeitsinn, Tübingen, 1868.

ments in which simple tone series were played on the piano. The use of the piano brought in certain changes, such as the appearance of tones in the place of noises, the use of not only one but of all the fingers, etc. The series of experiments corresponded exactly to that used in the investigations on free tapping. With the exception that the variations were somewhat smaller the results in all cases showed the same general characteristics as in the previous tests. Neither in the tapping nor in the playing of simple rhythms did the variations all lie in the same direction—some were positive and some were negative.

The experiments made for the purpose of finding out the effect of an accompaniment on the tempo consisted in having a certain portion of a piece of music played with and without accompaniment, and in every case taking the playing-time with a fifth of a second watch. In playing a passage repeatedly it was found that the variation in the amount of time required for the different performances rarely exceeded 0.2 to 0.3 seconds on an average. In playing 19 selections, nine of which were given 20 trials each, seven, 16 trials, and three, 10 trials, less time was required for the performing of each selection when played with accompaniment than when played without acccompaniment. Ebhardt explains this result on the ground that feeling enters in as the determining factor of tempo; an accompaniment increases the emotional effect. A number of the selections were also played upon a dumb clavier. In every case more time was taken than had been required for playing the same pieces without accompaniment. This retardation is attributed to two causes,— an increase of psychical activity, which consumes an excessive amount of time, and to a retardation in the rise of the feelings which do not come forth so quickly as with the full tonal sensations. The retardation is not noticed for the reason that consciousness has no means of perceiving absolute rate.

In a carefully conducted set of experiments Shaw and Wrinch[1] sought to determine by means of tapping movements "the degree of accuracy that obtains in the reproduction of complex rhythmically arranged groups of intervals of unequal length." From their results it appears that the average mean variation, when expressed as a percentage for all the reproduced groups, was only a fraction of one per cent. greater than it was for a series of simple intervals, and that the average mean variation of the complete groups was quite uniform, and smaller than it was for the individual intervals of the more complicated groups. In the repeated reproductions of a series

[1] University of Toronto Studies, Psychological Series, 1899, p. 44 ff.

the different lengths of the intervals held to one another within certain limits a comparatively uniform relation—there seeming to be a tendency to increase rather than lessen the accuracy of their reproduction as the number of judgments became larger. The length of the intervals was not affected by the differences in the pitch of the tones forming the melody.

In the records of ten intervals from the "Boccaccio March" there appeared four distinct types of notes, but their lengths did not hold to one another "the definite relation of one, a half, a fourth, and an eighth, as in written musical compositions." This leads the investigators to affirm that while their experiments yield nothing conclusive upon the subject yet they do indicate that "the relative length of musical notes in the production of a true musician is not in the exact ratio of one to a half, a quarter, an eighth, and so on."

General Summary of Literature. While the investigations cited have not been reports of the time values given by practiced musicians to the notes of complete musical compositions, yet they have relation to intervals and groups of intervals occurring in musical rhythms. A brief summary of the results that are important in connection with the present investigation may be made in the following statements: Musical rhythms contain elements additional to those found in simple rhythms and are therefore more complex. In music rhythm does not hold the primary place, but a co-ordinate place with other distinctively musical factors. Irregularity of movements in playing results from peculiarities due largely to the structure of the hand. Much of this irregularity may be overcome by practice. Irregularity is the most marked in rapid playing which leads to the expectation that in the production of a piece of music those measures that contain many notes will possess greater variations than those containing only two or three. Pitch of tones has no effect on the relative length of intervals. For simple intervals without accent the greatest exactness of execution is between 0.4-0.7 seconds. Accent, increasing the irregularity of the time relations in the group, is the source of a constant error. Usually the accented member is lengthened. In the production of complicated groups of intervals of unequal length the judgments are somewhat less uniform than for simple intervals, the individual intervals of the groups showing a greater variation than the complete groups. The time relations for the different reproductions of repeated groups of intervals show a tendency to remain quite constant. When a selection is played repeatedly there is little variation in the total time required (Ebhardt). The degree of accuracy which a musician secures in the production of intervals in the relation of one, a half, a quarter, and so on, is to be attributed

to the facility with which rhythmic movements become automatic (Meumann). An accompaniment influences the tempo by decreasing the total time. In the rendering of a piece of music musicians probably do not produce the tones in the exact ratio denoted by the written notes (Shaw and Wrinch).

Preliminary Experiments.

The Time Intervals in Selections Played by a Music Box. The purpose of this preliminary investigation was to discover the nature and extent of the variations in the duration of the tones in music produced in this very mechanical way. The music box used was of Swiss make, of medium size, and played ten pieces. The method employed was to count the number of half turns made by the fan regulator between the sounding of the individual tones when the mechanism was made to move very slowly. To facilitate this labor a mechanical counter was used which recorded each half turn of the regulator as it was allowed to take place. The number of half turns in the duration of the tones in the pieces studied were counted in this way three times. The counts for the same note sometimes varied one or two half turns in the different counts, but generally not at all.

Two pieces were thus measured, "The Blue Alsatian Mountains" and "La Gitana Waltz." Both are written in ¾ time. The music box in each case played only the principal theme to which, however, certain notes not found in the piano score were added. In reckoning up the results these variations of score have not been considered. The results are given below in half turns of the fan regulator, a half turn being equal approximately to 0.0074 of a second.

The part of "The Blue Alsatian Mountains" played by the music box contained 27 measures. The average length of the measure was 89.30 with M. V. of 2.28. The greatest difference between any two consecutive measures was 10. The average irregularity[1] was 2.73. In the part of "La Gitana Waltz" played there were 16 measures. Their average length was 78.13 with M. V. of 2.27. The greatest difference between any two consecutive measures was 9. The average irregularity was 3.73.

The variation in the notes of the same kind is also well marked. The average length of the notes of the different de-

[1] The "average irregularity" is found by averaging the differences *between the successive measures*, and gives a result somewhat larger than the mean variation, found in the usual way by averaging the variations of the individual measures *from their mean*. The "average irregularity" has been used here and below, because it is believed to be a better index of the particular sort of irregularity in question.

nominations is as follows: In "The Blue Alsatian Mountains:" quarter note, 30.73 with M. V. of 1.83; half note, 58.42 with M. V. of 1.42; dotted half note, 86.83 with M. V. of 1.83. In this piece there was no more than one note of any of the other denominations. "La Gitana Waltz:" quarter note, 25.67 with M. V. of 1.44; eighth note, 14.25 with M. V. of 2.25; dotted quarter note, 34.25 with M. V. of 1.12; half note, 54.0 with M. V. of 2.0; dotted half note, 78.20 with M. V. of 2.64. It is interesting to observe that in "The Blue Alsatian Mountains" the notes of the denominations other than a quarter note are shorter than their proportionate value as measured by the quarter note, and that with the exception of the dotted quarter the reverse appears true in "La Gitana Waltz." Whether this is purely accidental or the result of design cannot at present be determined.

As regards the relative length of accented and unaccented notes nothing certain appears. Comparing the average accented note with the average unaccented note there is found in both pieces a very slight difference in favor of the unaccented note, but the probable error in both cases is so large that the difference has no significance.

The data which this study of the music box presents toward the solution of our main problem is not very great; but, since its selections may be supposed to be rendered in reasonably good time, it does give some indication of the extent of variation in the length of measures and notes that will ordinarily pass unobserved, and furnishes, as a very "mechanical" means of producing music, an interesting basis of comparison with the performers whose playing is next to be considered.

Time Relations in the Playing of Musicians.

Apparatus, Method, and Subjects. The instrument used was a small reed organ manufactured by Messrs. Moore and Moore, of London, and tuned according to the directions of Ellis for use as a Harmonical.[1] For rhythm experiments the organ has one important point of difference from the piano. Upon it the intensity of the tone does not vary with the intensity of the finger stroke.

It was necessary that the recording portion of the apparatus should meet the following conditions: 1. It should be free from all disturbing noises; 2. It must not modify the action of the instrument so as to interfere with the technic of the playing; 3. It should register accurately and return to its original position the instant the sound of the tone has ceased. Elec-

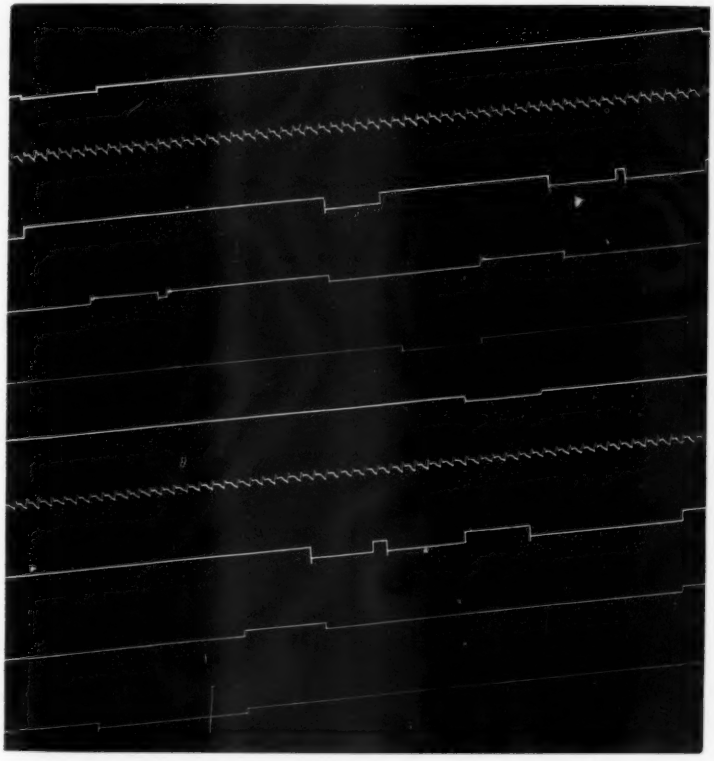
[1] *Cf.* Ellis's translation of Hemholtz's Sensations of Tone, Second Edition, p. 466.

trical registration by means of mercury contacts was employed, the records being finally traced upon the smoked surface of a kymograph drum. The contacts were arranged as follows: Steel pins were passed through the keys about two and a half inches from their back ends. Beneath the keys at the back of the instrument was placed a series of mercury cups in such a way that when a key was depressed the lower end of its steel pin would dip into the mercury and make an electrical contact. To the upper end of the pins were attached wires which led to brass buttons fastened to the back of the organ frame. From these buttons other wires led to four writing magnets, three Deprez and one Pfeil signal, which wrote on the smoked drum. It is obvious that with so small a number of writing magnets it would be impossible to record all the notes of all the parts in a harmonized piece of music. It was therefore decided to record only the upper or soprano part, that being the leading part and the one most likely to show the general time relations which were the chief matter of investigation. In order to secure even this with so few inscribing points it was necessary to arrange the wiring so that no two consecutive notes should fall to the same point. This was worked out for each selection by previous study, and wire connections were especially arranged each time for each selection by attaching or detaching from the brass buttons above mentioned. As the apparatus was finally arranged three notes could be assigned to each magnet, though as a general thing this was not required. An example will make the matter clearer. In one of the pieces used there are ten different tones in the soprano part, viz., d', $e'\flat$, f', g', $a'\flat$, a', $b'\flat$, c'', d'', $e''\flat$. The keys corresponding to these tones were distributed to the four writing points as follows: To point 1. $e'\flat$, a', $e''\flat$; to point 2. d'', g', d''; to point 3. $b'\flat$, f'; to point 4. $a'\flat$, c''. By reference to the selection (No. 254, below) it will readily be observed that in its execution the keys connected with the same point were never depressed consecutively. Thus there was between any two functionings of a single point one or more records made by one or more of the others. By this means each note had a distinct place in the record, and thus the reading of the record was facilitated.

In reading the kymograms the notes on the different lines were read in the order in which they occurred in the selection played. For example, in the specimen record here reproduced the transitions from one line to another were made as follows: 1 to 2, 2 to 3 (two notes), 3 to 4, 4 to 3, 3 to 2, etc.

In addition to the four points already mentioned a fifth wrote simultaneously a time-line in tenths of a second from which values in hundredths could be read by estimation.

Before each subject began playing such adjustments were



carefully made as should make it certain that the recording of each note would begin at the instant the key was sufficiently depressed to produce the first sound of the tone.

It was thought that the usual way of blowing the organ with the feet might act as a disturbing factor in that it would itself require a certain rhythmic movement or possibly a partial division of attention, either of which might interfere with the subject's rendition of the music to be played. To avoid such effects the organ was arranged for hand blowing by an attendant. This method obviated the difficulties mentioned, but proved not absolutely noiseless. The subjects, however, reported themselves as uninfluenced by this noise. Care was taken to work the bellows irregularly so as to prevent the sounds from furnishing any regular rhythmic effects.

Screens hid from the subject's view all the recording apparatus and as far as possible the movements of the attendant who worked the bellows.

The music selected for the investigation consisted of church hymns. Five selections were made, each of which contains some characteristic feature peculiar to itself. (See the scores in connection with the tables at the end of the article.) In most cases the subjects played the air only, and three records were taken successively of each hymn. In case of two of the hymns, all four parts were also played, the air alone being recorded. In these cases two records only were taken.

Before taking any records the subject had ample opportunity to familiarize himself with the action of the organ and the selections to be played. He was given perfect freedom to play in his own natural way. Records were taken from four different persons, all practiced musicians. B, M, and W were church organists. B and M were professional musicians of considerable local prominence, teachers and chorus directors. S, the wife of the writer, has been a music teacher and a church organist, but at the time of the experiment was somewhat "out of practice."

The first examination of the records disclosed the fact that very rarely did one tone begin at the instant that the preceding one had come to an end, but that one tone usually began before the preceding one had ceased. This occasioned what may be termed an "overlapping" of tones. Sometimes, however, an interval occurred between two successive tones instead of an overlap. In counting up the records it was therefore essential to report two things, namely, (1) the time during which the note sounded, and (2) the length of the overlap or interval by which it was connected with the preceding and succeeding notes. In the final reckoning up of the lengths of the notes it is necessary to take these overlaps and intervals into account, and this has been done throughout on the assumption that each note was intended by the player to last until the following note was struck and no longer. The overlap has therefore been subtracted from the full time of the preceding note in every case in which it appeared and the interval added when it occurred.

Results of Tests in which the Air Alone was Played.

General Rate. The time occupied in playing the whole hymn or selection is of course a slightly variable one. It not only varies for different subjects in playing the same selection, but also for the same subject in playing the same selection at different times. The results below give in seconds the average

time of the three trials for the different subjects and the different hymns.[1]

TABLE I.

Showing in seconds the general rate when the air alone was played.

	SUBJECTS.			
Hymn.	S	B	M	W
18	28.20	31.33	23.83	23.57
53	18.32	18.75	18.41	16.83
254	38.87	36.54	——	——
464	33.20	35.01	21.26	——
516	——	——	41.39	38.09

The greatest difference between any two subjects for hymn 18 is 7.76; for hymn 53, 1.92; for hymn 254, 2.33; for hymn 464, 13.75; and for hymn 516, 3.3. In a comparison of the average times of the different subjects for playing the different selections it appears that it is a general characteristic of W to play at a faster rate, and of B to play at a slower rate than either of the others. Beginning with the one having the least rapid rate the subjects stand in the following order: B, S, M, W. In hymn 464 no satisfactory explanation can be made for the great difference 13.75. M in all three trials played this special selection at an unusually rapid rate.

The greatest difference existing between any two consecutive trials by the same subject and with the same selection is as follows:

TABLE II.

Showing the greatest difference between any two successive trials.

	SUBJECTS.			
Hymn.	S	B	M	W
18	.17	2.16	1.35	.79
53	.38	2.21	2.73	.82
254	1.17	2.74	——	——
464	.54	1.87	1.03	——
516	——	——	1.27	.[71][2]

[1] For hymns 254 and 516 records were taken from two subjects only. In the tracings for W., hymn 464, the relations of the writing points at starting could not be determined with sufficient exactness, and the records were rejected.

[2] One of W's records with hymn 516 could not be used in this case.

Subject S in the case of hymn 464 and subject M in the case of hymn 516 performed one of the trials on a different day from that on which the other two were played, with a difference in time of 2.78 for S and 3.14 for M.

On account of the size of the drum of the kymograph only one trial could be recorded at a time for most of the selections. Thus it was necessary before each repetition to pause long enough to substitute another drum or to apply and smoke a new paper. In the case of selection 53, however, it was possible to make two records on the same drum, and there was thus only a brief pause between each two of them, with a pause of the usual length between the twos. On comparison of the length of the three trials in the case of the other hymns it was found that the first trial was the shortest 7 times out of 9; the third trial was the longest 4 times out of 7; and the second trial took more time than the first trial, but less time than the third 7 times out of 10.[1]

In hymn 53, where two records were taken with practically no intervening pause, every subject required less time for the first trial than for the second. In the case of B a second set was counted in which the reverse appears true, but the combined time expressed by the two records in this set exceeded that of the first set by 4.88. These facts would indicate, so far as they have a general significance, that a musician in playing a selection a second time tends to play it at a slower rate than he did the first time, provided no pause or only a short one lies between the original performance and the repetition. This result is the opposite of that found by Triplett and Sanford[2] for the recitation of nursery rhymes.

The differences appearing in Tables I and II are of such a nature as to elude explanation except as "personal differences," though a fuller study of the individual subjects would probably account for some of them.

Bach and his school tried to establish an absolute standard of rate, taking as a typical measure a group of four quarter notes, one to every pulse beat. To this measure the time values of all others bore a constant ratio.[3] But such a system as this, if strictly carried out, would be as apt to increase as to decrease such irregularities as are here under consideration; and modern musicians having set aside the normal pulse beat, find their

[1] There are not the same number of trials throughout for the reason that in one instance one subject gave the first and third trials the same amount of time, and that in two other instances one of the records was taken on a different day from that of the other two.

[2] *American Journal of Psychology*, Vol. XII, pp. 376, 382.

[3] Kufferath: Rhythm, Melody and Harmony, *Music*, XVII, 37 f.

absolute standard in the metronome or in the number of quarter notes per minute.

The Measures.

The measures vary not only with the general rate but among themselves for the same subject and the same selection. The following table gives the average time of the measures in the different selections. Here and throughout the remaining part of the work partial measures at the beginning and at the end of a selection are not taken into account; and the last full measure, owing to its being affected by the approaching close of the piece, is also excluded.

TABLE III.

Showing in seconds the average length of the measures.

	SUBJECTS.			
Hymn.	S	B.	M	W
18	3.43	3.65[1]	2.94	2.81
53	3.08	3.16	3.07	2.66
254	2.39	2.23	——	——
464	2.07	2.18	1.34	——
516	——	——	1.75	1.59

Examining the measures as they appear in the individual trials the greatest difference between any two successive measures may be found. These differences are stated in the following table. The figures in smaller type below the differences represent the value of the differences as a percentage of the corresponding average measure in the preceding table.

From these amounts the differences of successive measures fall away to zero. In order to indicate the average amount of variation we may calculate the "average irregularity as explained above." The results are given in Table V.

There is no clearly evident order in the variation of the measures except a retardation when nearing the end of the

[1] Average of six measures. In every trial B made a disproportionately long interval (pause) at the end of the first half of the selection. If this interval, which amounts to .36 sec. is added to the preceding note, as has been done in the case of all other intervals, the time of the full measure becomes 4.65 seconds. If this measure is taken into account the average length of a measure for B with this hymn becomes 3.80 instead of 3.65 seconds.

TABLE IV.

Showing in seconds the greatest difference between any two successive measures in the individual trials.

	SUBJECTS.			
Hymn.	S	B	M	W
18	.25 7.3	.29 7.9	.18 6.1	.18 6.4
53	.23 7.4	.31 9.8	.12 3.9	.27 10.2
254	.27 11.3	.17 7.6	——	——
464	.24 11.6	.25 11.5	.17 12.7	——
516	——	——	.23 13.1	.39 24.5

TABLE V.

Showing average irregularity.

	SUBJECTS.			
Hymn.	S	B	M	W
18	.096	.128	.072	.073
53	.066	.098	.082	.140
254	.095	.058	——	——
464	.099	.057	.053	——
516	——	——	.088	.174

selection. In the final averages the last full measure is longer than the next to the last 12 times out of 15; it is equal once and smaller twice. The next to the last is larger than the one preceding it 9 times, equal twice, and smaller 4 times out of 15. If the measures in the separate trials are examined then the last full measure is longer than the next to the last 35 times out of 47, equal 3 times and smaller 9 times; and the next to the last is larger than the one preceding it 29 times, equal 3 times and smaller 15 times.

The general irregularity seems to be greater in the last half of the selections, but this is due chiefly, if not entirely, to the terminal retardation already mentioned. As regards other possible relations the most that can be said is that in some of the hymns there is a suggestion of a systematic arrangement in the length of the measures within the groups allotted to the lines of the hymn or within the corresponding musical phrases.

This takes the form in two of the hymns roughly of a slowing from first to last within the line or phrase group and in a third of a progressive quickening. In the other two cases no such relation can be made out. One of the latter was played from a manuscript score, and the words were unknown to part of the subjects. The evidence furnished by the tracings for such an accommodation of the music to the words is too slight to warrant the assertion that it exists, but, nevertheless, such a relation is not impossible.

The Notes.

Since the time of the measures is variable, it follows that the time of their component parts must also vary. Such variations are found not only in the different measures, but even in the same measure. The variation of the notes of the same denomination in different portions of the same selection need not be discussed here. That they do differ and how widely may be seen in the tables at the end of the article, where the average time values are given together with the musical score. The table below gives the average length of the various kinds of notes irrespective of their place in the selection.

TABLE VI.

Showing in hundredths of a second the average length of notes.

HYMN.	SUBJECT.	WHOLE.	DOTTED HALF.	HALF.	DOTTED QUARTER	QUARTER	EIGHTH.
18	S			87		41	
	B			92		44	
	M			74		37	
	W			71		35	
53	S			111	80	51	22
	B			109	79	53	23
	M			101	76	52	24
	W			96	67	45	16
254	S			123		59	
	B			110		56	
464	S		156			51	
	B		162			55	
	M		100			34	
516	M	174		86	70	44	22
	W	136		82	66	41	20

If the quarter note is taken as the basis of comparison it appears that the relative length of the other kinds of notes differs for the different subjects, and with the exception of S for the same subject in different selections. They are sometimes too

long and sometimes too short. All the subjects make the eighth notes too short, sometimes very much too short, except M in hymn 516, where the ratio is exact. S, also without exception, makes the notes of higher denomination than the quarter too large. B, M, and W are less uniform than S, varying from the standard in both directions. Another method of testing the same matter—applicable in certain cases—consists in finding the number of instances in which the notes of other denominations are proportionately longer or shorter than the average quarter note of the same measure. The results of such a comparison are, however, with few exceptions in agreement with those just given.

Special Cases.

Triplets. In hymn 18 there are two triplets, one in the second full measure and the other in the last. Both take the place of unaccented half notes. All the subjects but M make the first triplet shorter than the average unaccented note, and all but S make the second longer. This last, however, is probably due to the approaching close of the piece. If each triplet is compared with the unaccented note of its own measure the results are conflicting, and the chances seem about equal that the triplet will be too short or too long.

Turning to the individual notes of the triplet there is a tendency to give more time to the second note than to the first, and more to the third than to the second. In the average results the second note in both triplets is made longer than the first by S, M, and W. This is not true of B, however, in either triplet. If the separate trials are examined, there are six possible chances for each subject to make the second note of the triplet longer than the first. S, M, and W do so 5 times, and B 3 times of the six. The last note of the triplet is made the longest of the group by all the subjects, and in all cases with the exception of M in the case of the last triplet where there are two trials in which this is not true. Thus while the tendency is slight to make the second note longer than the first, it is very strong to make the last longer than either of the others.

Dotted Eighth and Sixteenth Notes. The combination of dotted eighth and sixteenth notes in the second measure of the measures from hymn 53 presents an interesting study. All the subjects without exception made the whole measure very slightly longer than the ones that precede and follow it, though with such slight differences that the relation may be only accidental. Three of the subjects gave more time to the half of the measure containing the dotted eighths and sixteenths than to the first half. S made it 0.02 sec., B 0.08 sec., and M 0.03 sec.

greater, while W made it 0.01 sec. less. A comparison of the component parts of the last half of the measure with the average of the quarter notes in the first half of the measure shows a great lack of uniformity in the duration of both the dotted eighths and their combination with the sixteenth notes. On the other hand the sixteenth notes with two exceptions are all of too long duration. B and M each make the first sixteenth too short, but in both of these cases the dotted eighth with which the sixteenth is coupled is unduly long—longer than its proportionate part of the standard unit of measure, and longer than any of the other dotted eighths.

Accent.

Meumann, in his experiments upon rhythms executed in bare taps, found a marked tendency to hold the tap corresponding to the accented beat of the measure (*op. cit.*, p. 321), and Ebhardt reports something similar (*op. cit.*, pp. 126 and 127). The question naturally arises whether such a tendency appears in the musical records under discussion. Not all the selections contain such combinations of notes as to make determinations possible, but the number is sufficient to throw some light upon the question.

TABLE VII.

Showing in fractions of a second the average length of the accented and unaccented notes.

Hymn.	No. of Usable Notes.		Subject.	Average Accented.	Probable Error.	Average Unaccented.	Probable Error.
	Accented.	Unaccent'd.					
18	13	10	S	.893	.0053	.836	.011
			B	.926	.0063	.901	.011
			M	.744	.0043	.711	.0065
			W	.715	.0051	.690	.0063
254	22	22	S	.589	.0028	.591	.0040
			B	.557	.0036	.559	.0025
516	27	25	M	.454	.0033	.427	.0023
			W	.418	.0041	.404	.0048

Table VII gives the results for the trials in which the air only was played. In hymn 18 when all parts were played (see p. 38 above and the tables at the end of the article) there were 20 notes whose accent could be determined. 12 of these occupied positions of accent. The average duration of the accented notes was .898 sec., with a probable error of .0078, and that of the unaccented notes was .858 sec., with a probable error of .0099. These figures from the tests with all parts

are for subject S alone. For reasons that will be given later no results could be computed for the tests with other subjects.

Comparing the lengths of the average accented and unaccented notes for each subject in the different selections it will be noted that in hymns 18 and 516 the duration of the accented note in every instance exceeded that of the unaccented one. Hymn 254, on the contrary, does not show this characteristic. Neither in the case of S nor of B is there any more than a small fraction of a thousandth of a second one way or the other. Accent does not seem to have affected the relative duration of the notes in this hymn at all.

The above computations were made upon note values corrected for the length of time the preceding tone overlapped the succeeding one (*cf.* p. 38). For purposes of comparison the average accented and unaccented notes have been calculated without this correction. This was done for all the subjects and for all three selections. The results, although showing the same general characteristics are far less uniform than before, the greater variation being caused by the great differences in the lengths of the overlaps. In hymn 254, however, there appears a slight tendency in the case of both subjects, S and B, to lengthen the accented notes which was not the case, as has already been noted, when the overlaps were subtracted in the usual way.

The relative lengths of the accented and unaccented notes may also be studied by counting in the individual records the number of times the accented note exceeds in length the unaccented note immediately following it. The results are expressed in tabular form in Table VIII, the count being made upon the corrected notes used in Table VII.

If the results for all the subjects are combined there are in hymn 18 one hundred sixty-eight accented notes. Of these 116 are made longer, 6 equal, and 46 shorter than the following unaccented note. Hymn 53 has 72 usable accented notes, of which 38 are longer, 6 equal and 28 shorter. Hymn 254 has 132 accented notes, with 60 longer (18 of which occur at the end of the stanza lines), 15 equal, and 57 shorter. Hymn 464 has 90 usable accented notes, with 43 longer, 11 equal, and 36 shorter. Hymn 516 has 162 accented notes, of which 99 are longer, 11 equal, and 52 are shorter than the following note.

It is evident from the foregoing that accented notes are often longer than unaccented notes of the same denomination, but it is also evident that this tendency is not present in all cases and with all players. One may conjecture that it will appear most prominently in pieces, *e. g.*, marches and processionals, which require a strong marking of the rhythm, and will be more or less completely absent in pieces of a more flowing character.

TABLE VIII.

Showing relation of accented notes to the immediately following unaccented notes.

HYMN.	SUBJECT.	WHOLE NUMBER OF ACCENTED NOTES.	NUMBER LONGER.	NUMBER EQUAL.	NUMBER SHORTER.
18	S	42	29	2	11
	B		30	—	12
	M		30	2	10
	W		27	2	13
53	S	18	12	1	5
	B		8	—	10
	M		8	2	8
	W		10	3	5
254	S	66	29	8	29
	B		31	7	28
464	S	30	15	4	11
	B		18	2	10
	M		10	5	15
516	M	81	54	5	22
	W		45	6	30

Intervals and Overlaps.

As already explained on p. 38 there were found in the original tracings many instances in which one note ceased to sound before the next began, and many also in which the first overran the beginning of the second. The first I have called "intervals," the second "overlaps." Both deserve a little fuller consideration.

Intervals. The intervals are of two sorts: 1. Those which occur between consecutive notes on the same degree of the staff, involving of course the same digital of the organ. 2. Those which occur between successive notes on different degrees of the staff. The first is much the larger class. Where two notes occur in immediate succession on the same degree of the staff a minute pause is necessary in order to make the two distinct. Such pauses occurred with all the subjects and in equal number.[1] The number of intervals of the second kind varies for the different subjects and for the different repetitions of the selections.

[1] This, however, would not be true when playing all parts of the selection, for organists quite generally keep the key depressed during the whole time denoted by the several notes on the same degree of the staff.

TABLE IX.

Showing the number and average length of intervals of the first kind. Unit 1 second.

HYMN.	NUMBER OF INTERVALS.	AVERAGE LENGTH.			
		S	B	M	W
18	24	.084	.102	.071	.081
53	18	.131	.111	.050	.079
254	18	.138	.113	—	—
464	24	.078	.095	.099	—
516	54	—	—	.608	.088

TABLE X.

Showing the number and average length of intervals of the second kind. Unit 1 second.

HYMN.	S		B		M		W	
	NUMBER.	AVERAGE LENGTH.	NUMBER.	AVERAGE LENGTH.	NUMBER.	AVERAGE LENGTH.	NUMBER.	AVERAGE LENGTH.
18	13	.137	3	.017	0	0	11	.032
53	11	.128	3	.023	0	0	6	.015
254	15	.178	4	.057	—	—	—	—
464	7	.208	0	0	0	0	—	—
516	—	—	—	—	7	.023	14	.029

It is to be noted that the length of an interval does not depend upon the kind of notes in which the selection is written—that the intervals are no longer in a piece written in half notes than they are in one written in quarter notes. Subject S in most cases made intervals of the second sort to correspond with the pauses at the end of the verse lines, but this is characteristic of S alone.[1] These intervals are usually much longer than those of the first kind, and explain the great length of the intervals of S, in Table X. The other intervals of the second sort are short, and are probably accidental as they do not occur at corresponding places in the different trials. The characteristic of S in making intervals at the end of verse lines does not, however, seem to have affected correspondingly the time of the measures in which they occur, and this is one of the facts that make it uncertain whether the words really modified the time of the measures as explained above.

[1] There is one exception to this statement. B in hymn 18, as already explained, p 41, made an exceptionally long interval at the end of the second verse line. Its average for the three trials is .363 sec.

Intervals have something of the nature of rests; they have little effect in increasing the length of a measure. The note just preceding the interval is shortened, and to be equivalent to a full note it must have added to it the value of the interval, as has been done for all the preceding tables.

Overlaps. Consecutive notes on different degrees of the staff are generally overlapped. This is probably done in the interest of smoothness. It is nearly impossible to make the end of the one tone and the beginning of the next exactly synchronous; and, if it were, the effect would very likely be less flowing than with the overlaps. Intervals on the other hand would give a staccato effect not desirable without special reason.

The overlaps are by no means uniform. Perhaps all that can be said is that if an overlap is large in one trial of any subject it is likely to be found so in the rest. The same two notes, however, may not overlap to the same extent even in different parts of the same selection. For instance, in hymn 254 *g'* and *b'* flat occur as consecutive notes three times, holding the same position in the measures and making in the course of the three trials nine cases where *g'* overlapped *b'* flat. The lengths of these overlaps given in hundredths of a second are for S, 1st trial, 6-7-8; 2nd trial, 5-5-4; 3rd trial, 8-9-3; and for B 1st trial, 6-3-4; 2nd trial, 5-5-6; 3rd trial, 8-6-6. In like manner *c''* precedes *b'* flat three times, the length of the overlaps being for S 1st trial, 7-5-5; 2nd trial, 4-4-5; 3rd trial, 3-4-5; and for B 1st trial, 6-5-6; 2nd trial, 5-4-5; 3rd trial, 6-3-3. From the comparison of a large number of cases it does not appear that the number of degrees of the staff intervening between any two successive notes affects to any appreciable extent the length of the overlap.

The average overlaps for the three repetitions of the different selections have been calculated, and are as follows:

TABLE XI.

Showing number and average length of overlaps in fractions of a second.

	S		B		M		W	
HYMN.	NUMBER.	AVERAGE LENGTH.	NUMBER.	AVERAGE LENGTH.	NUMBER.	AVERAGE LENGTH.	NUMBER.	AVERAGE LENGTH.
18	71	.044	80	.051	90	.064	63	.030
53	70	.043	78	.049	82	.057	72	.026
254	124	.050	130	.044	———	———	———	———
464	91	.048	97	.053	96	.050	———	———
516	———	———	———	———	152	.063	133	.031

As to the length of overlaps different subjects present different characteristics. M's average overlap in every case is twice as long as W's, which is shorter than that of any other subject. The kind of note in which a piece is written does not appear to affect greatly the length of the overlaps. Their average length in hymn 18, which consists mostly of half notes, is even less than it is in some cases where the selections are largely written in quarter notes.

It should be recalled that a note generally has two overlaps, being overrun by the preceding note at the beginning, and itself overrunning the following note at the end. From what has already been stated in relation to the small difference in the length of overlaps for long and short notes it is easy to see that in the case of notes of the larger denominations the length of the notes, minus their overlaps, is often proportionately greater than that of such small notes as eighths and sixteenths when similarly treated. It frequently happens with an eighth or sixteenth note that the part of the tone wholly free from overlaps is very small indeed. In the second measure of hymn 53 M gave to the first sixteenth note, the g', a total time of 0.18 sec., and for the sum of the overlaps at the two ends 0.13 sec., leaving only 0.05 sec. in which the note sounded alone. These figures represent the average of the three trials, and furnish an instance where the overlaps were exceptionally large in proportion to the length of the note. For the same note W, whose average overlaps are small, gave for the whole duration of the tone 0.14 sec., for the overlaps 0.05 sec., and for the time of the independent duration of the tone 0.09 sec., thus using but little over one-third of the time for the overlaps, and this is more nearly a typical case.

The tests in which all parts were played show in the soprano part the same characteristics as to overlaps as those in which the air alone was played. It is altogether likely that in playing all parts of a selection the tones in every part overlap one another in a manner similar to that in which they overlap in the air, though not necessarily to the same extent and at the same time. That this is true at least for tones struck simultaneously the records furnish some evidence. In those selections where all parts were played, several alto notes were of necessity recorded in connection with the soprano notes, but rarely did the tones in the two parts in such cases begin and end at the same instant. A few examples taken from the individual trials will suffice for illustration. In hymn 18,[1] second

[1] This hymn was transposed from the key of G to that of B flat. The tones here named are those found in the selection as it was transposed and played, the tones g', d'', b' flat corresponding respectively to the tones a', e'', c''' of the selection as it was originally written.

full measure, first note, the g' in the soprano was held by S 0.05 sec., and by M 0.12 sec. longer than the d'' in the alto. In the third full measure, first note, S held the b' flat in the soprano in one case 0.03 sec., and in another 0.08 sec. longer than the d'' in the alto, while B held the d' in the alto 0.06 sec., and M 0.02 sec. longer than the soprano b' flat. In the sixth full measure, first note, W began the d' in the alto 0.03 sec. before the g' in the soprano, while B began the soprano g' 0.01 sec., and S 0.02 sec. before the alto d'. The last note of the last triplet sounded longer than the d'' in the alto in different instances as follows: 0.04, 0.05, 0.09, 0.04, 0.07, 0.15 sec. For hymn 53 the records contain similar instances.

Results of Tests in which All Parts were Played.

As already mentiond Meumann has conjectured that the use of both hands in playing may result in greater precision in keeping the rhythm, and Ebhardt (*op. cit.*, p. 147) has found that the use of both hands quickens the general rate of execution. My own experiments furnish a certain amount of data on both points. Two hymns (18 and 53) were played by all the subjects, first as airs only, one hand being used, and then complete, both hands being used in the customary way. Each hymn was played three times as an air, and twice in the complete form.

General Rate.

The average time for playing the different selections was as follows:

Table XII.

Showing in seconds general rate when all parts were played.

	S	B	M	W
Hymn 18,	27.21	32.14	25.29	23.07
Hymn 53,	18.68	21.76	18.64	18.15

In hymn 18 the c' key was not connected with the writing magnet, hence the tone c' which occurs twice in the air of this hymn was not recorded, and the average time for playing the whole hymn is really larger than that given in the table by the time of two half notes. But even so if these times are compared with those required when the air alone was played (Table I, p. 39 above) it will be seen that in two cases more time was taken to play the selection when all parts were played. This is the reverse of Ebhardt's experience, and points to the need of further investigation of the matter with a view to discovering the effect of special conditions, and the range of individual differences.

The difference in the time of the two trials which the differ-

ent subjects gave each piece when playing all parts is expressed in tabular form as follows:

TABLE XIII.

Showing in seconds the differences in rate when all parts were played.

	S	B	M	W
Hymn 18,	.74	.87	1.62	2.91
Hymn 53,	.41	.15	.46	.59

A comparison of the two trials shows that in hymn 18 the second trial was the shortest and the first trial the longest every time, and in selection 53 the first trial is shorter three times and longer once. In case of hymn 18 there is thus a contradiction of what was found in this regard where only the air was played.

The Measures.

The average length of the measures is larger than where the air only was played, and necessarily so since the total time is greater.

TABLE XIV.

Showing in seconds the average duration of the measures.

	S	B	M	W
Hymn 18,	3.52	3.99[1]	3.25	3.28
Hymn 53,	3.12	3.59	3.12	2.82

The following table gives the greatest variation between any two successive measures occurring in the individual trials. The figures in smaller type below each number represent, as in the case where the air alone was played, its value as a percentage of the correspondiug average measure in the preceding table.

TABLE XV.

Showing in fractions of a second the greatest variation between any two successive measures in the individual trials.

HYMN.	S	B	M	W
18	.30 8.5	.19 4.8	.24 7.4	.23 7.0
53	.32 10.3	.15 4.2	.16 5.1	.27 9.6

[1] B presents here the same characteristic as was observed where the air alone was played (see foot note, p. 41), namely, that of making a very long interval at the end of the first half of the selection (the middle of the stanza). If this interval, which is .56 sec. long, is added to the preceding note according to the regular method of treating intervals then the length of the measure within which it falls is 5.21 sec. Reckoning in this measure the length of the average measure becomes 4.19 sec.

For the most part these differences are larger than when the air only was played; in case of subject B alone are they smaller. The average irregularities are as follows:

TABLE XVI.

	S	B	M	W
Hymn 18,	.095	.108	.108	.121
Hymn 53,	.155	.090	.080	.204

If these irregularities are compared with those for the same hymns when the air alone was played it will be found that the irregularities are on the whole somewhat greater when all parts are played.

The deviations of the individual measures from the average measure present the same general characteristics as when the air alone was played, but there is frequently a lack of correspondence in the signs of the deviations in the two ways of playing. The variations are also considerably larger where all parts were played. The deviations in connection with the fourth measure in hymn 18 are all plus, and show that the measure was retarded by all the subjects. The first half of the stanza ends within this measure, and it is within this measure, also, that the transition takes place from one line of the score to the next lower. In hymn 53 there is one set of deviations in which the signs are all minus, and one in which they are all plus, traces apparently of a tendency to begin each line of the verse or phrase of the music rapidly and then to retard. This is clearer in the trials with all parts than where the air alone was played.

The Notes.

The following table gives the average length of the different kinds of notes that are found in hymns 18 and 53 where all

TABLE XVII.

Showing in hundredths of a second the average length of the notes where all parts were played.

HYMN.	SUBJECT.	HALF.	DOTTED QUARTER.	QUARTER.	EIGHTH.
18	S	88		44	
	B	100		52	
	M	82		42	
	W	77		42	
53	S	119	75	51	26
	B	120	88	60	30
	M	98	87	53	18
	W	——	72	49	26

parts were played, and is to be compared with Table VI, p 43. In finding these averages all doubtful[1] notes were excluded.

A study of the relative values of these notes shows just as great variations as are to be found when the air alone was played. So far as can be judged from the half and quarter notes there is a tendency here to shorten the longer notes and lengthen the short ones In hymn 18, for example, when the average half note in the tests with all parts is compared with two average quarter notes S made the half note equal, B 0.04 sec. shorter, M 0.02 shorter, and W 0.07 shorter than two quarter notes. When only the air was played S made the half note 0.05 sec. longer, B 0.04 longer, M equal, and W 0.01 longer. A like comparison of the notes in hymn 53, where comparison is possible, presents similar relations.

Special Cases.

Triplets. The triplet groups in hymn 18, so far as the records could be used, present no striking differences from those in the same hymn when the air only was played. B, M, and W in the first triplet connected the first note with the preceding half note, thus making it impossible in case of these subjects to ascertain the whole duration of this triplet or the value of the first note. In all cases the last note of the triplet was given more time than the second. A similar characteristic was noted where the air alone was played. (See p. 44.) S, B, and M made the second note of the last triplet shorter than either of the others, while W made it longer than the first and shorter than the third. The variations here do not correspond in their relative positions with those that occurred where the air alone was played, and they are upon the whole somewhat larger.

Eighths and Sixteenths. The last half of the second measure of hymn 53 was more irregular in the tests with all parts than when the air alone was played. In all cases the part of the measure containing the combination of dotted eighths and sixteenths exceeds the amount of time given to the other half. The excess for the different subjects is as follows: S 0.11 sec., B 0.10 sec., M 0.07 sec., W 0.05 sec. The difference here is greater than when the air only was played as may be learned by referring to page 44. This relative lengthening of the shorter notes in the "all parts" playing is in harmony with the tendency already noted in the section on the notes, though the case is not entirely clear. Comparing these elements with the average of the quarter notes in the first half of the measure

[1] Occasionally an alto note was recorded in connection with a soprano note. In such instances the length of the note could not be determined with absolute accuracy.

we find that S and M made the eighth notes proportionately too short, and the sixteenths too long, while B showed a strong tendency to make both kinds of notes too long, and M was equivocal. Both B and M gave the second sixteenth note, that is, the tone *b'* flat, no separate individual existence. This was true for both subjects in each of the trials. The tracings show that the second eighth note, or tone *a'*, overlapped the third eighth note, or tone *c''*. In the case of B this overlap was 0.03 sec., and in the case of M it was 0.04 sec. The tone *b'* flat appears to have been played at the same time as tone *c''*, beginning with it and ending before it. It is quite possible that in playing these notes the two keys were touched at the same time, the *b'* flat key being fully depressed and the *c''* key only partially so until after the *b'* flat tone had received its full time, when the *b'* flat key was released and the *c''* key fully depressed. But the partial depression of the *c''* key was sufficient to cause it to sound and to give a record. This explanation seems to be verified by the great length of the *c''* tone for both subjects, which is not very different from the combined length of the first dotted eighth and sixteenth notes, that is, tones *a'* and *g'*.

Accent.

In the selection where all parts were played the conditions are not favorable for determining what is true in regard to the relative length of the accented and unaccented notes. The subjects in accordance with instructions played in their habitual way. Three of the subjects in playing two or more notes that followed one another consecutively on the same degree of the staff depressed the key but once, keeping it down for a time equal to the duration of the whole group of notes. It was impossible to determine the value of other notes because with the means used for recording they were not separated from notes preceding or following them in the alto part. In hymn 18 note *c'* was not recorded at all. Subject S (hymn 18) gives the only cases in which the records can be used for this purpose. The results have already been given on page 45. They show, like those where the air alone was played, that the accented notes had a somewhat longer duration than the unaccented ones.

General Summary of Results.

Throughout the experiments there is a wide range of "personal difference."

When there was no intervening pause, or only a short one, there was in most cases a considerable variation between any two successive executions of the same selection. In such instances the smallest difference in general rate for any subject

and for any selection is .17 sec., and the largest 2.74 sec. The tests indicate that a musician in playing a selection a second time is more likely than otherwise to play it at a slower rate than he did the first time, provided there is no pause, or only a short one, between the two performances.

The variations of the measures, also, are not constant. The difference between any two successive measures is frequently larger than 0.2 sec., while on the other hand it is often as small as 0.01 sec. There are, however, only three cases where the average irregularity amounts to 0.1 sec. The general irregularity is greater in the last half of the selections, but this is probably due to the terminal retardation.

The relative lengths of the tones were also variable, and do not follow exactly the ratios represented by the written notes. They were sometimes too great and sometimes too small.

A comparison of the triplets with the note occupying the other unaccented position shows that the chances were about equal as to whether a triplet was given too much or too little time. There was, however, a slight tendency to make the second note of the triplet longer than the first, and a strong tendency to make the last longer than either of the others.

In two of the selections containing such combinations of notes as to make determinations in regard to accent possible, there is a marked lengthening of the accented notes. In the other selection this characteristic is not present.

Intervals occur between successive notes on the same degree and on different degrees of the staff. Those of the latter sort are comparatively few, and, with the exception of those coming at the end of verse lines, are very short and probably accidental. When an interval occurs the preceding note is usually shortened and the length of the measure is not affected to any great extent. The number of overlaps is large; their length is far from uniform; and, like that of intervals, does not depend upon the length of the notes involved. In case of such small notes as eighths and sixteenths only a small proportion of the tone is without overlaps. The records indicate that in playing all parts of a selection the tones in every part overlap one another.

The results of the tests with all parts may be summarized as follows: The subjects executed hymns 18 and 53 with no greater accuracy when all parts were played than when the air alone was played. In some ways the results show a greater lack of exactness. The tempo is no better observed, the measures show a greater want of uniformity, and the errors in making the length of the tones of the different denominations are as large and as numerous. Meumann's hypothesis of a motor appreciation aiding the musician in his execution of the frac-

tional parts of intervals is probably not far wrong, but my results do not support his conjecture that in playing with both hands one aids the other in giving to time intervals their correct relative lengths.

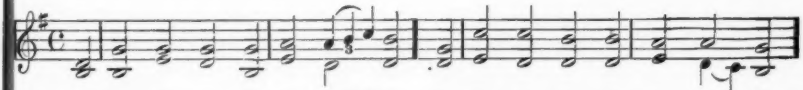
Below is given the scores of the selections that were played in the experiments.[1] In each case the average time values of the notes for the different trials are given in hundredths of a second.

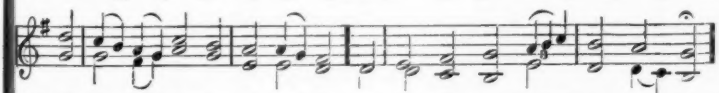
Hymn 18 was transposed from the key of G to that of B flat because of the special tuning of the instrument, and played from the manuscript score.

Air alone played.

18 All praise to Thee, my God, this night. L. M.

T. TALLIS.



S.	84	90	87	90	81	90	21	28	30	90	85	93	76	97	82	94	79	81
B.	91	96	91	95	89	84	25	24	40	89	86	93	89	96	85	92	84	201
M.	83	78	70	73	66	77	23	25	26	72	71	76	69	77	70	75	70	75
W.	68	73	67	72	65	74	19	20	28	71	73	73	68	73	65	73	68	76

S.	88	38	42	39	48	85	87	90	40	39	89	90	88	80	85	22	27	31
B.	88	46	44	43	45	95	94	94	42	42	91	98	91	97	94	27	25	41
M.	73	39	36	38	39	74	71	73	35	33	73	75	75	77	69	27	29	28
W.	70	36	34	34	35	70	68	66	34	35	71	71	71	74	67	22	22	29

S.	90	90	154			
B.	96	94	194			
M.	77	80	87			
W.	70	76	177			

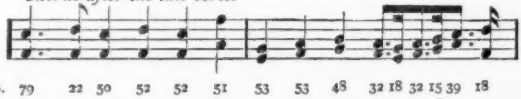
Air alone played.

Christmas

53 Shout the glad tidings, exultingly sing. P. M.

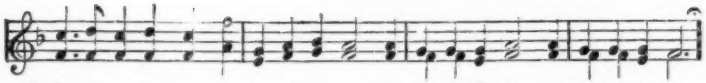
SECOND TUNE.

Chorus after the last verse.



S.	79	22	50	52	52	51	53	53	48	32	18	32	15	39	18			
B.	82	23	47	51	55	53	50	51	51	41	9	34	18	39	19			
M.	75	22	52	50	52	53	52	53	49	41	10	38	16	38	14			
W.	72	15	44	44	43	43	43	45	43	29	13	31	12	29	17			

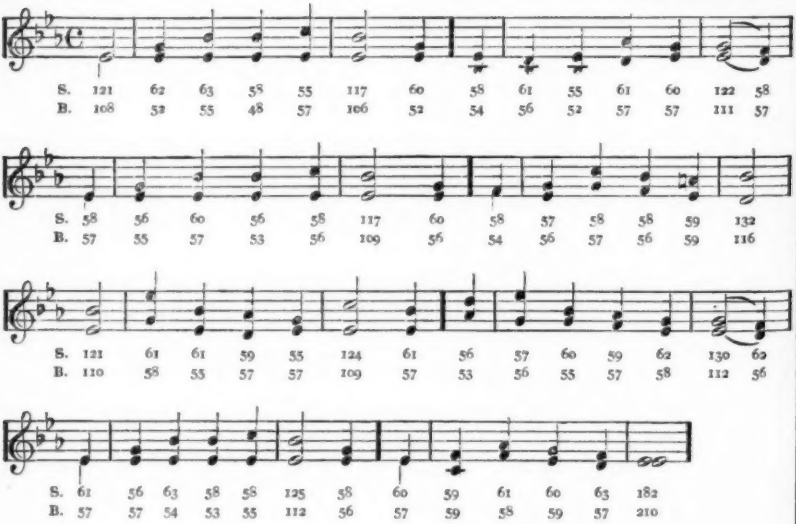
[1] For the electrotypes used, the writer is indebted to the courtesy of the Century Co., from whose Edition of the Hymnal the music was taken.

53.—Continued.

S.	81	22	49	51	51	50	52	49	54	111	46	57	52	48	110	47	57	53	53	129
B.	77	23	49	53	52	54	50	52	54	106	55	56	59	52	113	56	58	59	57	142
M.	77	25	49	48	51	52	51	52	51	101	53	57	50	50	101	56	55	50	53	146
W.	62	16	39	43	43	44	43	43	46	96	45	49	48	45	96	46	52	48	48	208

Air alone played.

254 From Greenland's icy mountains.

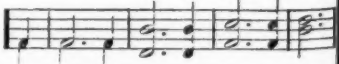
7.6. D.

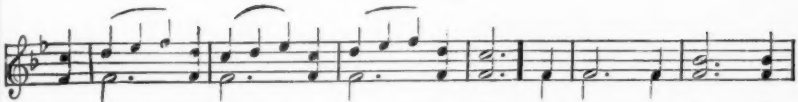
DR. LOWELL MASON.

S.	121	62	63	58	55	117	60	58	61	55	61	60	122	58
B.	108	52	55	48	57	106	52	54	56	52	57	57	111	57
S.	58	56	60	56	58	117	60	58	57	58	58	59	132	
B.	57	55	57	53	56	109	56	54	56	57	56	59	116	
S.	121	61	61	59	55	124	61	56	57	60	59	62	130	62
B.	110	58	55	57	57	109	57	53	56	55	57	58	112	56
S.	61	56	63	58	58	125	58	60	59	61	60	63	182	
B.	57	57	54	53	55	112	56	57	59	58	59	57	210	

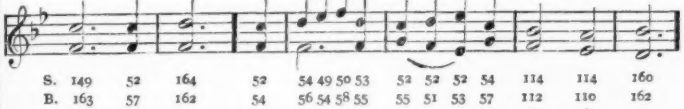
464 The spacious firmament on high.

D. L. M.

HAYDN.

Air alone played.

S.	54	152	50	154	47	151	48	156
B.	62	162	55	159	54	159	56	160
M.	38	102	31	106	31	102	32	98

464.—Continued.

S.	50	51	51	49	51	51	50	51	52	50	47	54	52	165	56	154	46	161	46
B.	54	55	55	57	54	55	54	53	55	54	50	59	56	163	58	165	56	162	59
M.	38	35	34	35	33	34	35	32	37	32	34	33	31	93	38	104	32	105	34

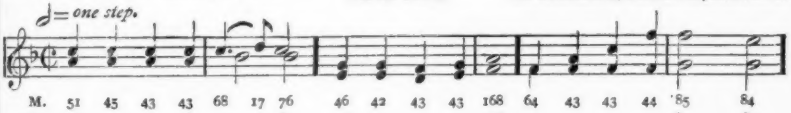


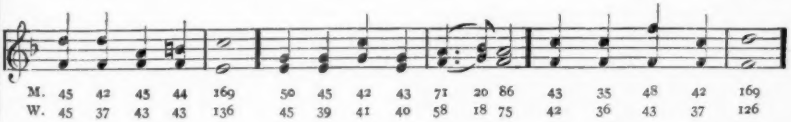
S.	149	52	164	52	54	49	50	53	52	52	52	54	114	114	160				
B.	163	57	162	54	56	54	58	55	55	51	53	57	112	110	162				
M.	99	34	93	33	34	34	37	30	33	33	32	35	63	69	79				

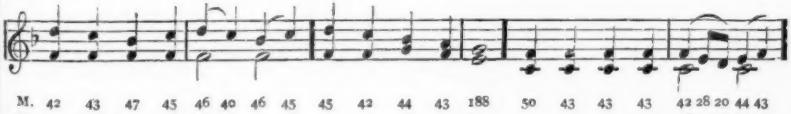
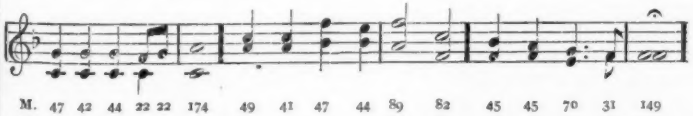
516 Onward, Christian soldier.

6.5.

Air alone played. FIFTH TUNE. Sir ARTHUR S. SULLIVAN, Mus. Doc.

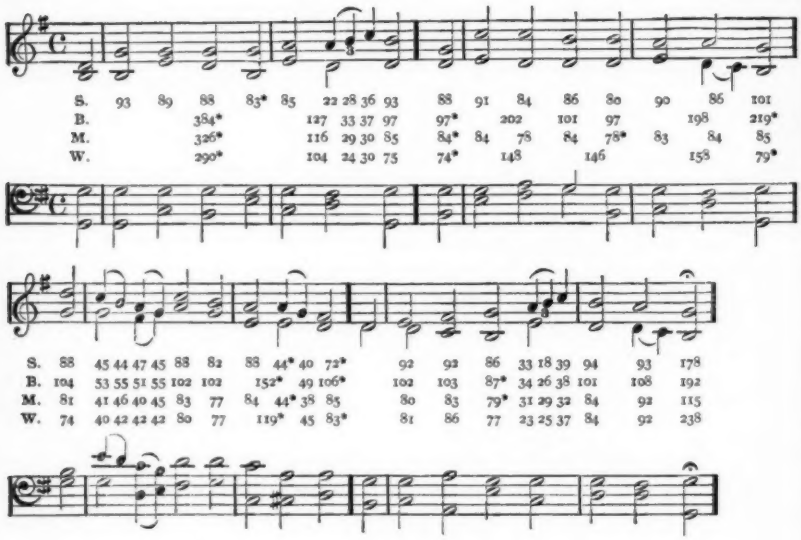
♩ = *one step.*

M.	51	45	43	43	68	17	76	46	42	43	43	168	64	43	43	44	85	84	
W.	48	49	45	46	70	19	82	46	39	44	42	128	41	42	40	44	82	85	



M.	45	42	45	44	169	50	45	42	43	71	20	86	43	35	48	42	169		
W.	45	37	43	43	136	45	39	41	40	58	18	75	42	36	43	37	126		

M.	42	43	47	45	46	40	46	45	45	42	44	43	188	50	43	43	43	42	28 20 44 43
W.	37	38	40	38	40	37	39	38	40	37	36	41	143	44	40	41	40	38	21 20 40 39

M.	47	42	44	22	22	174	49	41	47	44	89	82	45	45	70	31	149		
W.	42	42	36	20	21	146	45	39	41	44	86	82	46	46	71	23	157		

All parts played.

18 All praise to Thee, my God, this night.

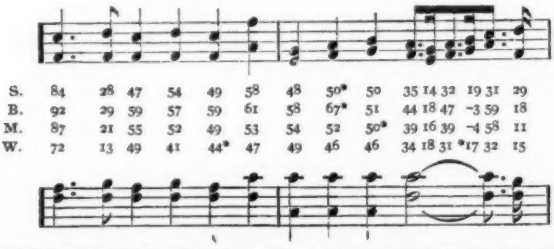
L. M.
T. TALLIS.

All parts played.

Christmas

53 Shout the glad tidings, exultingly sing.

P. M.

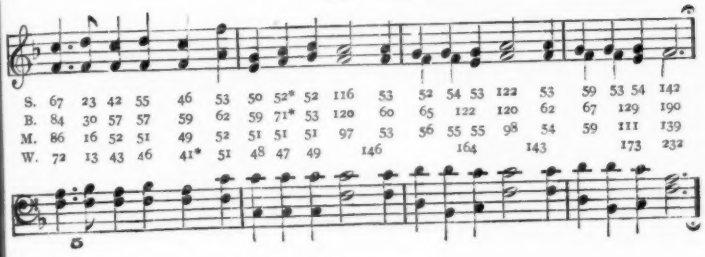
SECOND TUNE.



*The exact length of a starred note is doubtful owing to the fact that in being recorded it was not separated from the preceding or the following alto note.

The duration of the whole group of notes is given in cases where two or more notes follow one another consecutively on the same degree of the staff, and where the subject depressed the key but once for the whole group.

53.—Continued.



S.	67	23	42	55	46	53	50	52*	52	116	53	52	54	53	122	53	59	53	54	142
B.	84	30	57	57	59	62	59	71*	53	120	60	65	122		120	62	67	129		190
M.	86	16	52	51	49	52	51	51	51	97	53	56	55	55	98	54	59	111		139
W.	72	13	43	46	41*	51	48	47	49		146			164		143		173		232

RELIGIOUS EMOTION.

By H. B. Woolston.

Religion has been studied heretofore almost exclusively from the standpoints of history and of metaphysics. To these methods of procedure the recent interest in comparative religion has brought encouraging innovation. For only too long has the validity of a natural and universal sentiment been made dependent upon the historicity of certain documents, supposed to report the supernatural revelation of a particular faith, and upon the ability of a system of theology to demonstrate the finality of its dogma in the face of developing thought. Undoubtedly the historic origin of religious belief is of great importance in the study of its development. But who will tell us when we have come upon its first appearance? Surely the theoretical reconstruction of the primitive mind by latter-day philosophers betrays difficulties in the way of philogenesis, and the painstaking quarrels of historic critics should be a warning in the same direction. But whatever may have been the origin of religion, the fact that such a manifestation now exists is perfectly patent. No one can deny that there are millions of men all over the world who claim to hold religious faith, and who act in certain ways in evidence of this faith. Here, then, is a perfectly well established scientific fact: —religious faith does exist. Moreover, this faith manifests itself in certain phenomena that may be studied by us here and now. Therefore, just as the laboratory work of a student in modern biological science is considered of more importance for gaining a comprehension of the phenomena of life, than is mere acquaintance with the history of theories on the subject; so it seems to me that the study of the ontogenesis of religious faith by the collection and comparison of data from original and living sources, is of more importance for an understanding of the religious dynamic, than is labored exegesis or devotion to the history of dogma. By such method we may at least learn something new about religion, and escape the dreariness of mere philosophic comment. And perhaps, when we better understand how religion works in life, we may more wisely direct its activities.

Religion may be studied genetically from several points of view. Socially, religion manifests itself in ceremonies of the

cult and in certain institutional forms of activity. Intellectually, it has taken form in a body of more or less philosophical propositions called dogmas. But both these expressions in word and deed are objective, and more or less external to the real source of religious life. For modes of religious activity alter with changing civilizations, and systems of religious philosophy depend upon the intellectual culture of the people and of the time in which they appear. The constant element behind all these expressions is the religious experience as a fact of the inner life. This is the source of all external manifestations; this is the coustant center from which spring divergent expressions. Since men's minds are essentially the same, if we can find what are the functional activities that express themselves in religious phenomena, we shall have the key to the explanation of the latter. For, as Dean Mansel says, "Whether the relation of man to God be primarily presented to the human mind in the form of knowledge, or of feeling, or of practical impulse, it can be given only as a mode of consciousness, subject to those conditions under which alone consciousness is possible. Whatever knowledge is imparted, whatever impulse is communicated, whatever feeling is excited in man's mind must take place in a manner adapted to the constitution of its human recipient, and must exhibit such characteristics as the laws of that constitution impose upon it."[1] Religion for us, then, is a fact of inner experience to be described in terms of psychic activity, and to be explained according to the laws of our mind's functioning. For this purpose we shall use as our tool the analysis that psychology supplies, reducing the elements, so far as we can, to terms of physiology, in order that the results may be made as objective as possible.

From this delimination of our problem it is clear that we cannot consider the social side of religion. It is undoubtedly the case that the social environment of the individual has the greatest influence upon him, both in determining the character of his ultimate standard of life, and in directing his activities in striving toward his ideal. The *rôle* that imitation plays in the formation of character has been amply shown by Tarde and Baldwin. Moreover, on the intellectual side, there are certain phases of religious faith that we cannot discuss here. Such are,—the content and the logical relation of the ideas that accompany religious experience. A train of ideas necessarily accompanies such experience; but it is clear that the character and relation of these ideas will vary according to the nature of the individual's past experience. Such intellectual content must be assumed in the experience, else the activity would

[1] Limits of Religious Thought, p. 92-93.

have no sign to mark it off as religious. But it is not this side of the matter that interests us here. What we want to describe is the inner face of the experience; and, so far as we are able, the physiological machinery through which it is mediated.

Now it must not be supposed that we are confusing the experience as *inwardly felt*, with the processes of its functioning as *objectively described*. True, it is the same activity; but to the person who experiences it, the religious emotion has a value quite different from that in the mind of the observer, who merely analyzes its manifestations as a type of psycho-physical phenomena. Moreover, as to the nature and existence of the objects of religious worship, we have nothing to say. That is a matter for the epistemologist and the theologian. Our business is with the inner experience, as such. In short, our problem is to give some account of certain emotions that have religious reference.

First, however, we must give some account of the emotions in general, in order that we may have clearly in mind just that phase of psychic activity with which we have to deal. There are several theories as to the nature of the emotions, but most of them agree so far: (1) An emotion is the inward feel of an experience, the peculiarly personal sense of an activity. (2) An emotion is the psychic resonance accompanying certain trains of ideas and certain bodily activities.

In the affective life an emotion corresponds roughly to a train of associated ideas in the intellectual realm. That is, an emotion is more than a mere sensation. To use a figure, an emotion is not a point, but a line of pretty definite form. On the other hand, an emotion is less completely organized than a sentiment, which may be considered as a fairly constant state of feeling corresponding to a well established intellectual attitude assumed in the face of certain situations. Perhaps it may be thought that in consideration of this last definition we should have done better had we started out to treat of the "religious sentiment." But what we shall have to say on the lack of co-ordination in the average religious experience may justify our use of the middle term.

The main point in debate among the psychologists is as to the relation of emotions to bodily expressions. James and Lange hold that the body reacts in a certain way to a given stimulus, that the mind feels this state of tension or relaxation, and that this feeling is the emotion. The older psychologists hold that the mind feels in a certain way immediately upon receiving an internal or external excitation, and subsequently expresses its emotional state by nervous discharges. Neither of the theories gives a very intelligent account as to why the emotion expresses itself in just the way it does. Perhaps the

explanation of Professor Dewey would help out both at this point. He suggests (according to the first law of Darwin) that the nervous energies follow those channels which have been built up by the experience of the race in performing actions originally useful. As to the question at issue, it seems clear that genetically the physiological reaction does precede. Logically, however, the inner feel of an emotional stimulation must be appreciated, before the physiological accompaniments can be interpreted by the individual concerned, as expressions of his mental state. This doubtless is the point emphasized by the older psychologists. Personally, I should presume that it is the matter of sequence which Professor James is emphasizing, and not the identification of an emotion with its mere physiological accompaniments. At least the former point seems to me the only proper one for discussion. But if this interpretation be incorrect, and there is a more real dichotomy of mind and body involved in the views, then the remarks of Mr. Ribot apply. He says, both these theories are implicitly dualistic; one insisting on the causality of the physiological element, the other urging the precedence of the spiritual principle. But as a matter of fact, we know nothing of either element apart from the other. "Ce que les mouvements de la face et du corps, les troubles vaso-moteurs, respiratoires, sécrétoires experiment objectivement, les états de conscience corrélatifs que l'observation intérieure classe suivant leurs qualités, l'expriment subjectivement: c'est un seul et même événment tradint dans deux langues."[1] We do not say that physiology is psychology, any more than we should be inclined to assert that the inside of a bucket is the outside. But just as the changes in a muscle may be expressed in terms of chemistry or described as physiological action, so it seems to me the working of the human mind can be expressed in terms of psychology or in those of physiology. Such at least seems to be the practical assumption of most alienists.

But leaving the vexed question as to nature of the interrelation of mind and body, the intimate connection between states of body and states of mind is universally recognized. I quote from Nahlowsky:[2] "Wie innig das Gemüthsleben mit der Verfassung und Entwicklung des Organismus zusammenhängt, zeigt der Umstand, dass leibliche Gesundheit oder Krankheit, Geschlecht, Nahrungsweise, meteoroligische Einflüsse, Tages- und Jahreseit, ja selbst die Lage des Körpers (also samtlich Verhältnisse, welche direct die Leiblichkeit betreffen), mehr oderminderauch auf das Gemüthsleben ihren Einfluss üben."

[1] Psychologie des sentiments, p. 113.
[2] Gefühlsleben, s. 52.

The effects of alcoholic stimulation upon the emotions is a commonplace phenomenon. But on the other hand, the effect of the mind on the body is no less strong and immediate. Lange's *Gemüthsbewegungen* contains some excellent descriptions. The sight of a bear may be sufficient to cause my hair to stand on end, my eyes to stare, my jaw to drop. The blood leaves my face, my throat grows dry, my breathing is interrupted, my heart first stops and then pounds furiously, my abdominal viscera are relaxed, my limbs tremble, and a chill runs over my whole body. Two lines on a postal card announcing the death of a dear friend may throw me into a paroxysm of weeping and lower my bodily energies for many days. Just what this intimate connection between mind and body means we do not know. Personally, I think it means that *mind* and *body* are two terms used to express the activities of a unitary organism whose functions are regarded from standpoints designated by these words. Such a statement may merely dodge the difficulty involved in discussing the problem of interrelation. It at least avoids the suspicion of multiplying hypostases.

A word must be said as to the relation of the emotions to the process of ideation. When a stimulus affects the body there is a tendency to react immediately in a manner suitable to the structure of the organism. But if the situation cannot be met at once the afferent nerves transmit the feel of the matter to the brain. Here, in the neural switch-board of the body, the stimulus and answering impulse are thrown into relation with nerve currents reporting sensations and projective tendencies in other parts of the organism. All these partial impulses tend to fuse in a manner suitable to the nervous organization, usually over a resultant path found advantageous in the past experience of the individual. If this fusion be easily effected and the impulses reinforce each other, there will be comparatively slight consciousness of the resulting activity. But if there be a conflict between the tendencies, the organism is put to it to co-ordinate them or else be disrupted. In this way the situation is made objective, so to speak, to the organism as a whole. The partial activities must be turned into a larger circuit, and a new activity found. The perceiving of this more comprehensive activity is what we call the "imaging" or associative function of the mind. On the affective side, the feel of the energies that are welling up to be discharged in this larger activity is the emotion. Hence it will be seen that ideas must always accompany emotions, for the latter are only the resonance of the unco-ordinated forces that push the former into objectivity. It will also be seen, that as our ideas become far-seeing and our co-ordinations complete, there will be less emo-

tion, since there will be less internal friction to arouse affective resonance.

That religion on its inner side is a matter of feeling will scarcely be questioned. The work of Schleiermacher was to show this very clearly. He reduced religion in its essence to the feeling of absolute dependence. We should scarcely agree that this is a complete statement, but we cannot doubt that emotions such as fear, hope and joy play an important part in the development of the religious life. Perhaps the emotional element in religion will be sufficiently clear from a single quotation from von Hartmann.[1] He says, "Alle wesentlichen Momente des religiösen Processes gestalten sich zu psychologischen Zuständen, die wir nur als Gefühle zu bezeichnen vermögen; die Demuth der Endlichkeit vor dem Unendlichen, die Gottesfurcht, die der religiösen Weisheit Anfang ist, die Sehnsucht des Hertzens nach dem Göttlichen und seinem Besitz, diese folgenschwere Bewusstseinresonanz des noch mehr oder minder unbewussten religiösen Triebes, das gläubige Vertranen auf Gott und die selbtsverleugnende Hingebung der ganzen Persönlichkeit an ihn, die Qualen des Schuldbewusstseins und der Reue, die Verzweiflung des gottentfremdeten und mit Gott zerfallenen Bewusstseins, das Empfinden der von Gott gewährten Erlösung als einer wahr 'Erlösung,' das Durchkosten der tiefsten Erschütterung und der befreienden Erhebung, die Seligkeit der Versöhnung, die Gluthen der weltentrückten Andacht und die innere Sabbathstille des Gottesfriendens, dies und alles sonst nach zu Nennende sind ohne Zweifel nach psychologischer Klassifikation Gefühle." We do not intend to give a descriptive analysis of all these emotions in their religious bearing, but merely to present some account of how the more constant phases of religious emotion arise, how they manifest themselves, and what is their relation to bodily conditions.

First, let us see how religious feeling arises and develops in the individual. The religion of childhood and of primitive peoples is summed up pretty nearly in—fear of an external power, and hope of its assistance to overcome the disagreeable features of life. But with the growth of self-consciousness, there develops the sense of a more spiritual personal relation to the world about one. For as a man's sense of self develops, there necessarily accompanies it a perception of the not-self with which it is contrasted. And hence the necessity of establishing a working harmony between the two arises. Or perhaps it would be more nearly correct to say that a man's feeling of personality increases as, by growing knowledge of his

[1] Religions-philosophie, Bd. II, s. 28-29.

environment, he is led to relate himself to the system of things in which he is placed. It is clear that both sides of this relation develop together. The perception of such relation and the endeavor of the individual to satisfy his wants in accordance with the laws of the world about him, reveals to him a hierarchy of powers within himself. That is, the satisfaction of some impulses does not give the same complete and lasting result for him as does the satisfaction of others. Consequently the individual is led to set a higher estimate upon activities of the latter sort. It is found that the activities which effect a lasting satisfaction for the organism as a whole have greater worth than those which attain the ends of a partial impulse or a merely temporary desire. Thus the individual is led to identify himself with the most comprehensive processes rather than with partial manifestations, and to set up as his ideal the satisfaction that arises from complete functioning of all his powers. And so a scale of values is established in the mind of the individual. For as his various activities express his whole personality more or less completely, they are recognized as good or bad for him. Here, then, is a personal ideal without as yet any very clear ethical content.

But at the same time, the society in which the individual finds himself, prescribes that this ideal be of a certain type, and that the activities put forth in realizing it shall be directed along certain general lines. The principal feature about such a socialized ideal is its communal character. That is, the individual must not pursue his own selfish ends in a manner counter to the good of the group as a whole, but must realize himself as a member of the society, and must seek to advance the general welfare. This is a still larger ideal, which relates the individual to the social system, as his partial impulses are related to the development of his own personality. Certain activities of the individual that promote the common welfare are called "good," others that run counter to the functional activity of the group are termed "bad." The individual, by education and imitation is led to adopt this standard as his own, and to make it the test of his activities. Here morality appears.

But once more, the leaning of the group presents to the individual a certain *weltanschauung*, in which good and evil spirits or a personal cosmic principle are pretty sure to have an important place. The individual is supposed to sustain a relation to this cosmic society, just as he does to his own social group. Such relations are interpreted rather closely after the analogy of the code of the group; but the regulations of this cosmic society usually transcend those of the earthly community, and may even contravene group *mores*. Such, for in-

stance, is the case of human sacrifice in societies where manslaughter is proscribed. In this cosmic relationship, we have the largest whole to which the individual can relate himself. Consequently the law of this comprehensive order (interpreted as the will of God), is made the ultimate standard for all activities. It must not be supposed that this standard is an absolutely fixed one. It is rather the outermost ring of a developing view of life, such as we have attempted to trace schematically in the two preceding sections. A sense of this larger ethical and religious responsibility seems to develop at about the age of puberty. It is about this time that most conversions occur,[1] that confirmation is celebrated, and that the rites of initiation into the social life of the group are performed.[2]

The appreciation of this ultimate standard brings with it certain added responsibilities to the individual. He brings his personality to the test of this larger ideal, and estimates the worth of his activities by their place in the higher co-ordination. Hence any conflicts between his activities and what he conceives to be the universal order (interpreted as realizing the will of God), is felt as a sense of sin. The more persistent partial impulses that are likely to drag him off from striving toward this more general end are regarded as evil. And thus a dualism is often established between the lower animal desires and the more intellectualized tendencies (the "flesh" and the "spirit"), or even between the entire present order and the idealized system (the "world" and "heaven"). The consciousness of such an inconsistency leads to a state of inner tension that must be relieved by the perception of a new synthesis and the discharge of partial activities in a larger co-ordination. The solution of the situation is perhaps most strikingly illustrated in what is termed in evangelical Christian churches, conversion. And to a consideration of this phase of religious development we now turn.

Leuba[3] and Starbuck[4] have so well described this crisis in the spiritual life, that we can do little more than summarize their treatment. The sense of inward dualism that we have just described is heightened by depicting the hideousness of sin and its ultimate punishment. On the other hand, the blessedness of following the will of God is emphasized by numerous testimonies. The individual is led to identify all his partial, lower and selfish desires with an evil self; whereas his higher and more generous impulses are attributed to a gracious spirit

[1] Starbuck: Psychology of Religion.
[2] Daniels: The New Life, *Amer. Jour. of Psy.*, Vol. VI.
[3] Study of the Psychology of Religious Phenomena, *Amer. Jour. of Psy.*, Vol. VII.
[4] *Op. cit.*

pleading within him. The figure of Jesus is pointed to as the realization of the unity between God and man, and his acceptance as a saviour urged. The emotional power of music, reiterated appeal from the pulpit, the example of others, the pleading of friends are added, until the emotional pitch often becomes intense. The ideas in the mind at such a time are restricted in number, and being constantly emphasized, come to possess it with almost hypnotic force. There is often great mental anguish and bodily disorder at such a time. Finally the individual can stand the tension no longer. He yields to what he believes is the will of God—that is, he identifies himself with the more comprehensive unity. He breaks with the narrower co-ordination of activities that has hitherto bounded his life, and throws himself into the "great sweep of things." He is no longer the main center of the universe, but has his place in its spiritual system. The individual's will now being in harmony with God's will, and all his activities co-ordinated in a comprehensive scheme, he proceeds in life with new confidence. There is often a sense of newness and change after conversion, since the lately acquired co-ordination is not the one that has heretofore been identified with the self. There is frequently also a feeling of joy and strength, due doubtless to relief from tension and a new redistribution of nervous energy. We are well aware that such a description applies to a particular form of religious life, and that there are others wherein no such striking transformation is effected. But, nevertheless, it seems to be the case that the acceptance of religious belief marks the process of relating activities to a larger scheme of life. Such a co-ordination may come as a matter of gradual growth, or be marked as a crisis relieving a conflict of tendencies.

The recurrence of periods of doubt mark less pronounced manifestations of this essential process. The conflict here is generally of a less radical intellectual sort, and usually arises from the failure of the accredited religious *weltanschauung* to comprise and harmonize certain additional views or experiences of life. Here again the emotional tension may be great. The "old self" is identified with the accepted statement or customary form of activity, the "new self" with the broader view. The incorporation of the limited form of functioning into that recognized to be more comprehensive again gives relief and freshness. The stress of feeling in such periods is not generally due to distrust of the religious desire as such, but rather to dissatisfaction with the limitations of its accepted forms of expression.

There are certain types of religious life and thought in which the emotional element has asserted itself to an unusual

degree. This is especially the case in mysticism. The mystic is not satisfied to repose on any external authority, but would rise to a vision of God himself and hear the divine word within his own heart. "Pour cela, il est necessaire qu' il fournisse une préparation convenable; afin de s'élever aux choses du ciel, il faut que l'esprit se soit détaché de celles de la terre, et en outre qu'il se soit à la fois affiné et purifié par les austérités, la jeûne, les prières; d'autant plus que cet état cause des hallucinations, une prédisposition à l'inspiration. Si l'excitation est très grande et procure une sorte d'hystérie, l'esprit sera tout prêt à recevoir cette visite d'en haut. Elle viendra et l'homme sere inspiré, prophetisera, vera les esprits supérieurs, portera sur son corps les stigmates resultant de l'imitation par la volonte; il communiquera directment et extraordinairement avec Dieu."[1] It is true that this is a description of an exaggerated form of mysticism, but nevertheless of one that has been common to that tendency. The mystic goes apart from his fellow men, and seeks to find by absorption in the contemplation of God that unity which he feels is lacking in his life. The assertion of the common animal impulses is a temptation of the devil. The mystic would leave the prison house of his body, and rest in soul-to-soul communion with God. The exclamation of Maine de Biran—"Mon Dieu, délivrez-moi du mal, c'est-à-dire de *cet état du corps* qui offusque et absorbe tontes les facultés de mon âme"—is characteristic of a certain sort of mystic, and seems to point to a physiological origin of this tendency, *i. e.*, unharmonized functions striving for a point of unity and support.[2] Moreover, fixing the attention upon a limited number of ideas produces a state of emotion that corresponds to the nature of those ideas. This is the basis of the discipline of Loyola. This sort of self-hypnosis may lead even to an identification of the individual with the ideal form that absorbs his mind. In this way we may explain cases of stigmata. All stimuli are apt to be interpreted in the light of the controlling idea. Hence the frequency of illusions and hallucinations among mystics. It is highly probable that the organism of a saint, made sensitive by fasts and vigils, will respond readily to stimuli that would not affect a person in the ordinary condition. And it is very likely that many abnormal states of feeling will be objectively referred to spiritual causes.[3] If ecstacy or visions are of frequent recurrence or are accompanied by disease, we may expect to find a pathological condition as their immediate cause. Ste. Theresa, whose visions were most

[1] Grasserie: De la psychologie des religious, p. 262.
[2] Murisier: Le sentiment religieux daus l'extase, Rev. Phil., '98. Charbonnier: Maladies des mystiques.
[3] See Joly: Psychologie des saintes, ch. 3.

numerous, is known to have been epileptoid. However, we may not say that because men are susceptible to stimuli that we cannot appreciate, they are therefore abnormal in the bad sense of the word. Nor may we hold that a spiritual truth is useless because it was first discovered by a man whose mind was particularly sensitive in that direction.

There are two stages of mystic communion generally recognized by those who treat of this subject. The first is characterized by the perception of voices and visions, such as we have indicated. The second stage is that of ecstacy, or complete communion with God. This *ravissement* is often accompanied by motor impotence and sensory anæsthesia, so that there may be a feeling of levitation, or the soul may be felt to be exalted above the body.[1] It is an expansive feeling in which the distinction between the *me* and the *not me* is broken down, and the soul is wrapped in the Absolute. There comes a sense of oneness with God, untroubled by visions, unperplexed by obtruding ideas, which feels no fever of restless moral striving, but which is as a thrilling silence, as the throbbing of hearts in unison.[2] Since this stage is marked by the absence of clear ideas it cannot well be described in objective terms, but must be felt.[3] Sometimes the ecstacy is compared with the embraces of a consummated love. It will be seen that the emotional side of such *rapport* is very intense, whereas the intellectual and active phases are practically suppressed. It is all an inward quiver. There is another stage in the mystical development which brings it to its logical conclusion. It is simply "*aneantissement complet*," nirvana. There is no thought, feeling or action. Unconsciousness ensues, and the sense of personality is lost—"swallowed up in the Infinite."

The tendency of an emotional form of religion to adopt the practices of asceticism is well known. "They [ascetic practices] have been by all people adopted for the purpose of bringing on those abnormal states which are supposed to imply either possession by spirits or communion with spirits. Savages fast that they may have dreams, and obtain the supernatural guidance which they think dreams give to them; and especially among medicine men and those in training to become such, there is abstinence and submission to various privations with the view of inducing the maniacal excitement which they and those around mistake for inspiration."[4] The purpose of asceticism is to castigate or entirely suppress the less spiritual emotions. Distracting stimuli are shut off, and the attention is

[1] See Murisier, *loc. cit.*, Comp. II Cor. 12: 1-7.
[2] See Ruysbroeck: Du supreme degree de la vie interieure.
[3] See Godferneaux: Le sentiment et la pensee.
[4] Spencer: Principles of Sociology, III, p. 91.

fixed upon religious ideas. External and internal aids are employed to keep these ever before the mind. Striking passages from the holy books are conned; physical means are used to induce the proper state of mind. Flagellation and uncomfortable clothing seem to act as stimuli, by throwing the desired religious consummation into strong contrast with present misery. Fasts are observed by many faiths, especially before great religious festivals.

Perhaps a word should be said here about so-called miraculous cures, for such cases belong undoubtedly to periods of high religious emotion. It is possible, and very probable, that under stress of high excitement, the nervous forces may be gathered to overcome a functional disorder. It may also happen that under the suggestive influence of a religious rite the sufferer may temporarily disregard the disorder which later reappears.

The matter of revivals and of religious epidemics in general belongs rather to a discussion of social psychology than to a study of the emotions of individuals. We can only say, therefore, that an emotion is re-enforced by the perception of its social manifestation. A man may be convinced, a mob goes mad with conviction.[1] Perhaps we may dismiss this side of our problem by quoting from Granger.[2] "There is a natural exaltation of feeling when the mind directs itself upon sublime objects, but this is too often confused with the lower kind of excitement which is produced when human beings are assembled together in a crowd. This latter . . . . may be traced under the heads of contagion of feeling and hypnotic suggestion." A good or a bad emotion may thus be given momentum by gaining popular approval.

We now come to the question as to the relation of states of religious emotion to bodily conditions. From what has already been said of the emotions in general and of asectic practices in religion, it will be seen that the relation is a very close and real one. In fact we have held that states of body and states of mind are two descriptions of the functioning of one organism as regarded from somewhat different standpoints. Emotions are not entities controlling mind and body, but certain phases of the activities of these. Such being the case, the emotional states will vary with the varying conditions of the organism. And, hence, "religious states, as well as other states of mind, stand in reciprocal relation with states of the brain and nervous system."[3] As Mr. Joly says,[4] "Il me

[1] See LeBon: The Crowd, Bk. I, Ch. 4.
[2] The Soul of a Christian, p. 101.
[3] Coe: The Spiritual Life, p. 86.
[4] *Op. cit.*, p. 160.

paraît impossible de nier ce que l'emotion doit aux habitudes d'esprit, à l'education bein on mal dirigée du raisonnement, aux tendances de l'imagination, aux ideés, enfin et surtont aux croyances. Mais on ne peut méconnaître davantage ce que la sensation des troubles physiques des mouvements circulatoires et des divers actes réflexes interessant chacun de nos organs aux modifications des tous les autres, apportent de proptitude on de langueur, de calme on d'intensité aux emotions que nous travaillent." Hear what Ste. Theresa herself says: "The disquiet of the soul comes most frequently from bodily indisposition. . . . . The changes of the seasons and the alterations of the humors . . . often compel it to suffer in every way."[1] I venture to add yet another quotation from M. Murisier.[2] "La plupart des mystiques chrétiens reconnaissent que la misère, l'inquiétude, la tristesse de l'âme proviennent souvent de l'indisposition de l'organisme. Tant que notre âme est enfermée dans cette prison, elle participe à ses infirmités." It seems then that we have a pretty general recognition of the fact that religious emotions have an organic basis. And this we propose to demonstrate further from several lines of evidence.

(1) *The religious emotions are dependent upon certain constitutional and organic factors.* Mr. Coe has shown pretty conclusively by his investigations that the character of the religious emotions is dependent upon the temperament of the individual, and upon his susceptibility to suggestion.[3] Starbuck,[4] Hall[5] and Mercier[6] have demonstrated the dependence of the emotion upon the sexual condition. Indeed it is a well recognized fact that some sort of sexual mystery plays a part in almost every religion. But Starbuck has further shown the close connection between puberty and conversion. He also suggests that the ability to appreciate the very generalized relationships involved in religion is conditioned upon the completion of the third layer of transverse, or "associational" fibres in the brain.

(2) *Religious emotions vary in a fairly constant manner with certain meteorological changes and with bodily rhythms.* The evidence for this line of proof cannot be given here. The writer has been investigating the matter, and finds it to be the case. The results of the research, when completed, he hopes to publish at a future date.

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[1] Life, XI, 23.
[2] *Loc. cit.*, p. 461.
[3] Spiritual Life, p. 138 ff.
[4] Psychology of Religion.
[5] *Ped. Sem.*, Vol. I, p. 196 ff.
[6] Sanity and Insanity, passim.

(3) *The evidence of pathological conditions shows the connection between exaggerated religious emotion and diseased nervous states.* We may here remind the reader of the remarks made above under the heading of mysticism. Religious insanity has long been recognized. This is not generally attributed to devils nowadays, but is referred to a lesion or functional disorder of the brain. For a treatment of this phase of the subject we would refer to Kraft-Ebing,[1] Tuke,[2] Mercier,[3] Vallou and Marie.[4] We subjoin one case cited by Clouston.[5] He records a case of melancholia caused by amenorrhœa. The patient wept causelessly and bemoaned her fate as being a castaway from God. After five months treatment the patient menstruated, and recovery was immediate. The sense of religious depression and despair vanished, and religion did not trouble her one way or the other. Dr. Clouston adds, that persons of sensitive nerves and religious training are apt to hang their depression on a religious peg or doctrinal point.

(4) *Religious emotion is increased by the use of physical stimuli.* The employment of images and eichons to make the religious ideas clearer and to arouse the emotions of love, awe and reverence, has been common in practically every religion. Consider as examples, the figures of Budda, the crucifixes and pictures of the Passion in Romish churches. The use of music in religious worship has been almost universal. From the compositions of Handel and Bach to the war dance of the Dakotas we find the stirring effect of music employed to rouse the emotion of devotees. Perhaps such means of excitation, as well as the acts and attitudes involved in worship, may not strictly be called physical stimuli, since they lack that sort of immediate contact with an objective stimulant, which is frequently understood by the term. The forms we have mentioned, however, are doubtless physical enough in their source and in their appeal to the senses. But when we come to the use of incense and liquors to aid in exaltation, there is no doubt that physical means are being employed. The use of soma among the Hindoos to produce religious intoxication, and the inspiration of the Delphic priestess by means of inhaling gaseous fumes may be cited as instances. The use of the dance in religion to stir the feelings by rhythmatic action and to express the emotions in an expansive way is well known. The whirling dervishes and the ceremonial dances of the American Indians may be noted as examples.

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[1] Psyschiatrie, s. 85, 150, 326 f, 436 f, 521, etc.
[2] Dictionary of Psychological Medicine, art. Religious Insanity, etc.
[3] *Op. cit.*
[4] Des psychoses religeuses, Archives de Neurologie, 1897.
[5] Mental Diseases, p. 84 f.

In a very suggestive article in *The American Journal of Psychology* (Vol. VIII), J. H. Leuba defines the moral imperative as "a reflective, cerebro-spinal, ideo-motor process, unaccompanied (immediately) by sympathetic emotional reflexes." Hence we can understand the cold and "impersonal" character of the moral imperative, for it lacks that rich suffusion of organic sensations which we identify with the feel of our usual activities. The author suggests that the emphasis of various religions upon the "free spirit" as against the "heart of flesh," is nothing but the endeavor to keep the cerebro-spinal motor tracts free from the disturbing influence of the sympathetic system. If we comprehend all this, it is only a more psychological way of expressing what we generally admit—that those actions for which we feel a moral responsibility are those over which we exercise voluntary control; and secondly, that moral advance consists in progressively co-ordinating our blind impulses under intelligent direction. From what we have seen of the rich emotional accompaniments of religious experience, and in the light of these suggestions, we may appreciate with renewed force the psychological aptitude of Mathew Arnold's definition of religion as "morality touched with emotion." Religion thus involves the whole man; not only his well defined activities, but also the emotional presaging of larger coordinations that cannot yet be expressed in clear ideas or well directed effort.[1] *But the fact that intense religious emotion is accompanied by circulatory and visceral disorders,*[2] *and is marked by a paucity of clear ideas,*[3] *seems to point to the fact that nervous energies are being dissipated in the sympathetic system, instead of being profitably co-ordinated in the ideo-motor tracts.* While the vegetative processes are good so far as they go, it will be seen that domination by them is of distinctly lower value for the individual, and particularly for society, than is the execution of well directed voluntary activity.

What, now, are our conclusions as to the nature and value of the religious emotions? First, the religious emotions are the stirring of the forces deepest in the nature of man and inherent in the very organism of the human race. Religion is the voicing of a constitutional need, just as hnnger is the feeling of a constitutional need. It is the desire to maintain and perfect the personality beyond its present natural limits. The warranty for religion is thus not a matter of external authority nor of intellectual inference merely, but it is found in the very structure of man as a sentient animal. If this be to material-

[1] See Hylan: Public Worship.
[2] See Joly: Psychologie des Sanites, p. 164 f.; Murisier, *loc. cit.*
[3] Godfernaux, *op. cit.*

ize the spiritual nature of man, it is at the same time to spiritualize all of his nature. In this connection let me quote a passage from Professor James on the emotions.[1] "They are," he says, "Sensational processes, processes due to inward currents set up by physical happenings. Such processes have, it is true, always been regarded by the platonizers in psychology as having something peculiarly base about them. But our emotions must always be inwardly what they are whatever be the physiological ground of their apparition. If they are deep, pure, worthy, spiritual facts on any conceivable theory of their physiological source, they remain no less deep, pure, spiritual and worthy of regard on this present sensational theory. They carry their own inner measure of worth with them; and it is just as logical to use the present theory of the emotions for proving that sensational processes need not be vile and material, as to use their vileness and materiality as a proof that such a theory cannot be true." For my part, I should like to see the formal dualisms and watertight compartment systems of life done with, and life made a spiritual whole. Never will the higher nature of man be made the object of exact knowledge by super-refined and unreal methods of investigation. Only by painstaking empirical study can his experiences be rightly understood, and thus a sound basis be laid for the intelligent interpretation of his higher aspirations. Let me close such an apparently materialistic paragraph with the words of another.[2] "Shall we therefore conclude that conversion is practically an automatic performance? Not unless we first define conversion so as to ignore its profound relation to God and to the principles of a good life. If conversion is a *moral* renewal, it is not merely a psychical process of any sort. What has been proved is simply that, when conversion or an equivalent change takes place in one's moral attitude toward life and destiny and God, it may clothe itself in certain emotional habiliments provided certain factors are present, but otherwise not. The substance of religious experiences as far transcends their emotional forms as a man transcends the clothes he wears."

Let us next inquire what the office of emotion in the religious life is. The emotions as we have seen, are merely the psychic resonance of bodily or mental activities. They indicate the presence of unco-ordinated forces, and hence are especially marked on the breaking down of an habitual activity and the formation of a new co-ordination. An emotion is thus the stirring of forces that may be directed into a new and larger

[1] Psychology, II, Ch. 25.
[2] Coe: *Op. cit.*, p. 140.

activity. But in themselves, the emotions may be as unmoral as a fever chill or as useless as escaping steam. It is the turning of force into intelligently directed action in accordance with the needs of the situation, that makes the dynamic of value. It is the co-ordination of the dissipated energies into a consistent scheme of development that gives them moral worth. Consequently the religious emotion as such has no value at all, except as the inward resonance shows ready response of the nature to a certain sort of influence, and except as the excitation leads to a large and worthy effort. Indeed luxuriating in religious emotion is a distinctly immoral practice, comparable with physical indulgence and dissipation. To quote from von Hartmann,[1]—"Wenn die Religion wirklich eine gewisse relative Beseligung des in ihr Lebenden mit sich bringt, so ist das jedenfalls doch nur eine accidentelle Nebenwirkung, aber nicht der Zweck ihres Daseins; wer dieses Verhältniss auf den Kopf stellt und den individuellen Gefühlsgewinn, weichen das Individuum aus der Religion schöpft, an die Stelle ihres objectiven Zweckes setzt, der leiht damit der tiefsten Wurzel alles Bösen und Antireligiösen, der endämonistischen Selbstsucht, einen gefährlichten Freibrief, der schädigt die Entwickelung der Religion durch Konservirung und Stärkung der ihr aus der heidnischen Vorstufe noch anhaftenden endämonestichen Verunstaltungen." In other words, religious development is not to stop with a self-conscious gratulation at the pleasant stirring of new forces, but is to gain its real satisfaction by complete expression in proper activity in life. The weak and womanish aspect that the Christian religion has sometimes assumed from the undue emphasis upon the emotional side, has been amply scored by Coe.[2] So far as the emotion gives the enthusiasm of high endeavor it is good; so far as it becomes mawkish sentimentality it is execrable.

Finally, what is the practical value of religious emotion in life? As we have seen, the tendency of the religious development is to take a man out from the narrow bounds of his limited personality and relate him to the whole of things. The universal tendencies are emphasized as against the more restricted; altruism is urged in the place of egoism; the more spiritual activities are recommended as against the lower animal impulses; and the realization of moral endeavor is guaranteed by the righteous power of God. This description, of course, applies to a highly developed sort of religious teaching. But some such striving after union with a higher power seems to be involved in all the forms. The self will always be a center

[1] Religious philosophie, Bd. II, s. 52.
[2] *Op. cit.*

of activity, but not necessarily the only center, nor the main one. If I might employ a figure, I should compare the frankly egoistic attitude toward life to a circle, where all radii lead to the "self-center." The social type of mind might be represented by an ellipse, wherein the interval between the foci represents the extent of the social or patriotic interests. The religious order would then be typified by a parabola, where the focus is still the ego, but the directrix reaches out into the infinite. This last figure cannot be said to be an exaggeration even as applied to primitive religions. For though savages may not have a very clear notion of infinity (such as our philosophers are blessed with), yet the outer limits of their system corresponds to this conception—as among all self-respecting peoples. Höffding has called religion the "cosmic vital" feeling. And that to my mind is the essence of the matter. "A man comes to realize the spirit that moves within him as a part of the whole. And a feeling of this vital oneness gives a certain peace and confidence which is religious in its nature if not in its form."[1] A man feels that his will is identified with God's will; that his plan is a part of God's plan. And such a feeling of *rapport* with the source of all power gives a man immense energy to suffer or to do. The religious emotion especially arouses the expansive manifestations of love, to which it is very much akin. And thus it leads to unselfish activity. The value of religion in enforcing morality and thus forming a strong social bond, has been generally acceded. The sense of living under the eye of an "ideal socius," or of existing as a part of God's very being, cannot help but have an exalting effect on a man's relations with his fellows. "En nous présentant la religion comme la lien unificateur par excellence ou la cause origenelle qui consolide et maintient le group social, nos prédécesseurs ne se trompèrent qu' à demi; car à la religion pour contenu essentiel ils donnarent l'amour des hommes et l'amour divin, la forme la plus large et la plus pure de l'altruisme." [2]

[1] Stanley: Psychology of Religion, Psy. Rev., '98.
[2] Roberty: Le psychisme social, p. 47.

THE PSYCHOLOGICAL NECESSITY OF RELIGION.

By STEPHEN S. COLVIN, University of Illinois.

The future of religion is a problem which to-day is holding the attention of many thoughtful minds, and not without reason, for nothing can be more evident than that among the educated classes there is a general dissatisfaction with the present form of religious expression and content of religious dogma. The belief of our forefathers, simple but crude, is no longer found adequate, yet there seems to be nothing better to take its place. Many fear for the future, not knowing in what direction the evolution will tend, while others generalizing from the limited view-point of their surroundings, believe that a finality in this evolution has been reached, and that religion is to decay and finally die.

Of writers holding this latter view no one, perhaps, has given more forceful expression to it than Guyau.[1] He says: "Human beliefs, when they shall have taken their final form in the future will bear no mark of dogmatic and ritualistic religion. They will be simply philosophical." Again he writes of religion: "Born as it is of certain beliefs and certain customs, its fate is one with them."

As he regards religion in its inception as 'nothing more than an imaginative extension of human society, the explanation of things by a theory of volitions,' he concludes that its end will be a return to philosophy and morals.

In his belief concerning the approaching annihilation of religion as such and a substitution for it of speculative thinking and practical ethics, Guyau finds a wide concurrence. For example, Zola asserts that the world to-day is without mysteries. Empirical knowledge has taken the place of religion. Renan has a similar view, and replaces theology with science. John Stuart Mill sees the religion of the future satisfied with an enlightened morality. Even Henry Ward Beecher wrote, "Nobody ought to be called an infidel who sees in justice the great creed of human life, and who aims at an increasingly complete adjustment of his will to his moral sense."

On the other hand there are many who regard the religious consciousness as something fundamental and implicit, and although they conceive the possibility of any particular faith

[1] "The Non-religion of the Future," New York, 1897.

as passing away, they deny that religion as such can ever be eliminated or transcended. Count D'Alviella in the Hibbert Lectures for 1891 says that God may die as his thousand predecessors, Baal, Odin, Jupiter have died, and the Israelitish Yahveh must one day die, but what cannot die is the conception, enshrined in these names of a mysterious and superhuman power, realizing himself in all the laws of the known universe, revealing himself in the voice of conscience, and the spectacle of the world. It is needless to multiply examples. The radical difference between the two views set forth above is clear without expansion, and it is not the purpose of this paper to further emphasize them, but to enquire which of the two we may accept, and if neither as a whole how we may modify and harmonize them. In order to do this it will be necessary to consider more closely the essential nature of religion itself, that we may see if it has any psychological necessity for existence, or whether it is a phenomenon of human life relatively persistent and general but nevertheless transient.

But at the very beginning we are confronted with a difficulty. A logical definition of religion seems impossible, while even a descriptive definition is hard to formulate. At the outset we may, however, disregard certain attempts to explain religion by setting forth the phenomena which have attended its development. To declare that it originated in fetishism, animism or ancestor worship is to do little more than to name the occasions of its evolution, not to consider the underlying causes.

Turning to a more fundamental conception we may notice Edward Von Hartmann's view that the nature of religion and its source are to be found in egoistic eudaemonism. This statement, however, is so manifestly prejudiced that it need not be considered here. Neither can we assent to the rationalistic explanation of Wolff and his school who made religion logical thought concerning God. Kant's reduction of it to the basis of a moral maxim deprives it of vitality. Martineau's definition of religion as 'belief in an Ever-living God, that is, a Divine Mind and Will ruling the universe' is likewise too limited to include many of the phenomena that are usually classed as religious.

Both Max Mueller and Herbert Spencer find the essence of religion in the feeling of man for the infinite. Pfleiderer says that its roots "lie in the manner in which primitive man regarded nature and the emotions with which nature affected him." John Fiske says that the idea of God sprang from man's dependence upon something without him, and D'Alviella places its essence in the conception man forms of his relation with superhuman and mysterious powers on which he believes himself to be dependent. Finally Schleiermacher de-

fines religion as the feeling of absolute dependence. These latter views agree in emphasizing the feeling element in religion instead of the rational, and in placing man over against an irresistible power which holds his very being in its grasp. On the whole Schleiermacher's definition seems the most satisfactory, but it must be taken with certain limitations. The feeling element which is made all important cannot be merely subjective. As Principal Caird has pointed out there must be a criterion outside of feeling to which we can appeal. On the other hand we cannot agree with Caird that "to place the essence of religion in feeling is self-contradictory." The feeling in which Schleiermacher seeks to find the germs of religion is that of individual helplessness when confronted by the universal power without, but it implies knowledge and will; knowledge as to the proper relation which the individual shall sustain to the power without, and the will which seeks to realize that relation. With these limitations, then, Schleiermacher's definition is accepted by the writer of this paper, and it is now proposed to show, (1) that this definition accords with what is known about the origin and growth of religions; (2) that this feeling of absolute dependence is the essential feature of the great historic religions of the present and the past, and finally that man is so constituted by nature and environment that he can never free himself from this feeling of absolute dependence, and as a consequence must always remain religious in the sense of the definition, and hence can never transcend religion nor convert it into a mere system of philosophy or morals.

It is a widely accepted view among anthropologists that religion originated in fear, and the first worship was an attempt on the part of man to get on the right side of those powers which man conceived of as being above him. This fear seems to have been of two kinds, first the fear of the unknown, the biological reason for the evolution of which is easily to be understood, and second the fear of the terrible and destructive forces of nature, the storm, the flood and the scorching heat. Naturally these two motives for fear blend in many instances and cannot be distinguished. Interesting in this connection is the fact that the first real gods were generally connected with the sky, suggesting both mystery and power. In the course of time it would be natural for man to create not only gods whom he should worship through fear, but also those whom he regarded as helpful, and whom he could trust. Thus two emotions, fear and love, are to be found in all religions of a relatively high development, although without doubt fear is the more original of the two, and forms a larger share of all primitive beliefs.

The point to be principally noted in this connection is that

this conception of the origin of religions answers in part at least to the definition of religion which it is the purpose of this paper to defend. Such a religion as we are speaking of is essentially a feeling, and it is a feeling which objectifies itself, that is becomes an emotion, this emotion being based on the recognition of the dependence of man upon something without him. So primitive religion may well be defined as a feeling of dependence, though as yet the dependence has not become absolute. In order that it may become so another step in religious evolution is necessary. Man as purely naïve does not regard his dependence upon superhuman powers as any more absolute than he does his dependence upon his fellow men. It is only in a higher stage of reflection that he recognizes that he cannot flatter, cajole or get the best of his gods, as he does of his neighbors. Even in such a well developed religion as that of the Greeks this feeling of absolute dependence is slight in the earlier period of Mythology. Prometheus, the type of man rebellious and partially successful against the will of Zeus, is a product of unreflecting atheism.

Man originally was neither an optimist nor a pessimist, for both of these attitudes toward life presuppose reflective thinking. Yet we must regard primitive man as taking a real joy in life, and not becoming despondent under pain, for his consciousness had not been "sickled o'er with the pale cast of thought." But before he has reached a very high stage of development, a strain of pessimism appears, and in pessimism, relative or absolute, is true religion born. For when it is recognized that there are ills from which human beings cannot escape by their own efforts, then comes the feeling of absolute dependence. Were the world perfect, or even relatively satisfactory, there would be no need of religion. But it is not, and it is in this radical imperfection that, as Schopenhauer puts it, is found the metaphysical necessity for worship of the divine. Man, says Von Hartmann, first believed that happiness was attainable for him as an individual, but he found this to be a delusion, then he said, 'though I cannot be happy, my children may,' but this he found also untrue; then in his despair he created heaven. So have thought the pessimists; but in a similar strain writes the great optimist Hegel in regard at least to this life when he says: "All that awakens doubt and perplexity, all sorrow and care, all limited interests of finitude, we leave behind us on the bank and shoal of time. . . . . It is in this native land of the spirit that waters of oblivion flow, from which it is given to Psyche to drown all her sorrows; for here the darkness of life becomes a transparent dream-image through which the light of eternity shines in upon us." Even John Fiske cannot regard this

world as entirely satisfactory, and when he thinks of man in the light of his origin he cannot believe that this world is all. Life rests on mystery.

Turning to a consideration of the great historic religions we may first notice those of the East.

Among the deities of the Vedas stand forth Varuna and Indra, both deities of the heavens. Varuna especially was a god of mystery and power, a ruler whom none might oppose with impunity. Later in the philosophy of Brahmanism the Indian religion had passed beyond its unreflecting stage and plunged itself into mysticism. Man is completely dependent upon Brahma from whom he issued forth and to whom he must return. The world is viewed with pessimism. It is Maja,—illusion. Redemption is freedom from the thrall of the senses.

Buddha came more than five centuries before the birth of Christ, and presented even a more pessimistic view of the world, though he did not supplant in any way the Brahmanistic *Weltanschauung*. Life is an endless round of pain, to escape from it Salvation, Nirvana the *summum bonum*. The individual is nothing in himself, and absolutely dependent on the world principle.

> "The dew is on the lotus: rise great Sun!
> And lift my leaf and mix me with the wave,
> Om Mani padme hum, the sunrise comes,
> The dewdrop slips into the shining sea."[1]

Another Aryan religion, the Persian, has a pronounced strain of pessimism in it, with its world struggle between the good and the evil. Its fundamental character is well emphasized in the Book of Job.

The Greek religion as we find it in Homer is very naïve and childlike. The Greeks in the vigor of their youth and early manhood took too much joy in life to evolve a deep and powerful religion. They made their gods a little higher than themselves and intensely human, but above all—gods and men alike—ruled Fate from which none could escape. As civilization grew the priestcraft in the mysteries of Dionysius and Demeter developed a more spiritual worship, one calculated to stir the religious feelings to their very depths, and awaken the emotions of awe and fear in the dreadful presence of the Gods, as revealed to the neophytes through the trickery of the priests. The Greek enlightenment scoffed at the belief of the people, and the Sophists sought to do away with religion and morals alike, but Plato reasserted the worth of the divine and made the only reality that of the beyond, while the world itself was

[1] Sir Edwin Arnold: The Light of Asia.

but a realm of shadows. Here pessimism in regard to the worth of this life powerfully asserts itself.

Of the Roman religion not much can be said; it was in the later days at least mostly formal, but nevertheless its elaborate rites emphasized to a peculiar degree the dependence of the people on the power of the gods.

As the ancient civilization began to decay and as the world-despair grew, how inadequate proved mere philosophies; Stoicism, with its "renounce and bear," and Epicureanism with its grasping after pleasure. Both were thoroughly pessimistic, but neither offered any real escape from the evils of life. Then came Christianity and saved mankind from utter ruin, not by putting greater worth into this life, but by making the life to come the hope of the disconsolate.

The Hebrew religion from which Christianity grew represents the highest type of Semetic religions. "The fundamental trait of the Semetic belief in God," says Pfleiderer, "is that of his separateness from the world, oppositeness to man, who feels himself to be a powerless slave over against the overmastering deity." The Hebrew religion emphasizes this pessimism and dependence. No darker picture was ever painted of life than is found in Job and the Preacher. Yahveh, originally a storm god, is a being of vengeance and justice, but never of love.

In Islam Allah, is "an absolute almighty ruler, fearful in his anger, arbitrary in his rewards and punishments, his will incomprehensible and irresistible; he requires from men slavish subjection, and even this does not certainly secure his favor. . . To this dark view of the deity there corresponds a somewhat pessimistic estimate of the world; it is compared to a dung heap full of rotting bones; its misery is so great that it can only be surpassed by the tortures of hell."[1]

The religion of Christ in no sense does away with the pessimism of the Jewish faith in which it takes its roots, neither does it make man less independent of the heavenly power. In fact by doing away with formalism it makes the dependence more direct and subjective. It is not sufficiently recognized to-day what a deep strain of pessimism runs through the teachings of Jesus and the faith of early Christianity. This world and the kingdom of heaven were placed in inverse ratio, and it was only as the one was renounced that the other was to be gained. Further, Christ demanded an absolute renunciation, complete self-surrender and dependence upon God, and the early Christians understood his doctrine in this way. The world turned eagerly to Christianity because mankind was help-

[1] Pfleiderer: "The Philosophy of Religion," London, 1888.

less. It is to be doubted that had Christ appeared a few centuries earlier when the world was more optimistic, if the religion of the Nazarene could have received such ready support as it did. Christ came indeed in the 'fulness of time.'

The Christianity of the middle ages departed widely from that of the early centuries. Formalism took the place of the truer and deeper religion, though in æscetism and mysticism the spiritual fires were kept from going out.

It is hard to estimate the Christian life of to-day, but it is characterized by less of real dependence and surrender of self than that of earlier times. The feeling of the age, especially here in America, is not deep enough to turn the mind toward the transcendent. We are too much engrossed in the things of this world, too self-confident, too prosperous to think profoundly of the beyond. There is a great deal of talk to the effect that Christianity will be transcended because the world has progressed too far intellectually to longer accept the old-time dogma. To my mind the lessening of the religious life of the time is not a matter of intellect, but of feeling, and there is sure to be a rebound when a deep wave of pessimism sweeps over the world.

We have now reached the final question which it is the purpose of this discussion to consider; namely, granted that religion has been properly defined as the feeling of absolute dependence, can it ever be transcended or eliminated? The answer must be a decided no. And this for two reasons.

First, as we conceive it religion might be done away with if intelligence were perfect and knowledge absolute. If there were nothing left to be known, if all mysteries were cleared up, if we could see the whole plan of the universe unfold, whether it was for weal or for woe, then we might in the cold light of reason look at life without worshipping the power which controlled it. If its end were order and happiness then we might enjoy life in its fullness, and bear sorrow and pain with fortitude, seeing the good beyond; if, on the other hand, the end were mere nothingness, a return to original chaos, if all were without a plan and life without meaning, then we could become either Stoics or Epicureans. But an absolute knowledge is manifestly impossible. We can never know more than phenomena, and as the phenomena are infinite our knowledge must always remain infinitesimal. Too much value has always been given to intelligence. It is but a spark in the darkness. A large proportion of life is feeling, and we are always tending to lapse into the unconscious. Mere intellect can never hope to solve the mystery of the universe. Science when rightly interpreted has not removed mysteries, but revealed new ones. We know no more of the meaning of life or the destiny of humanity

than did Plato. Moreover, it is unthinkable that we ever can know. In the presence of the riddle of the universe we may not always pause and reflect, but when we do, when we remove ourselves from the whirl of existence, the eternal question of life's meaning must impress itself upon us. He who imagines that reason can answer the demands of the human heart, has never deeply and profoundly thought.

Secondly, religion might be dispensed with were life satisfactory in itself, but that it can never be. Pierre Lasserre (in La Crise Chrétienne, Questions d'Aujourd 'hui, Paris, 1891,) maintains that philosophy and science have failed to solve the meaning of suffering. "There is more truth in the profound astonishment of simple souls when a great blow falls on them, than in any other attitude spontaneous or reflective. In that may be recognized the intuition of a natural mystery."[1] Eradicate pain, death and sin from the world and religion will be needless; as long as they remain there must be a profound pessimism running through life, and it is religion alone which will give this existence value for the countless thousands who toil and suffer.

We must not look at man in his strength alone and judge life accordingly; if we do our view is limited and narrow. The fields of drudgery, the hospitals, the prisons, the almshouses, the places of mourning; we must consider them as well.

On a sunlit September afternoon the chief of a powerful nation stands full of life, energy and assurance. The future is bright, the present seems secure. A week later in the darkness and silence of the night, with life at an end and the death agony already upon him the heroic sufferer murmurs, "Thy will be done!" How weak is man, how absolutely dependent on the forces about him—the plaything of circumstance, the sport of fate! Human life, passing from mystery to mystery, attended by pleasure, but also by pain, is not sufficient unto itself.

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[1] Quoted from *Pedagogical Seminary*.

RHYTHM, TIME AND NUMBER.

By ROBERT MACDOUGALL, New York University.

The experimental researches on rhythm of Bolton, Meumann, Ebhardt and others have familiarized us with the idea of interdependence between the factors of time, intensity, number and position in the rhythmical sequence. They have corrected our apprehension of the nature of the absolute intervals which enter as constituents into the simple rhythm group, and have shown these to be no fixed units, but variables whose magnitude fluctuates with every change in the index of variation in the limiting stimuli. They have made known to us that the time-relations of the sounds which compose music and verse are neither simple nor constant, that the proportions presented by successive notes or syllables are vastly more complex than the score or the metrical scheme indicates, and that their ratios vary with every change of emphasis, expression and mood in the speaker.

The plastic character of their elements commonly escapes our notice in the rhythms of speech and song. The changes of which we are conscious are those of verbal meaning, interpretation and emotional tone. It requires an attitude of artificially discriminative attention to discover even the variations in stress and tonal quality which are involved in the production of such effects upon the hearer, while the concomitant changes of temporal relations in emphasized words, in sustained and slurred notes, in significant and insignificant pauses, cannot be brought to light by any device of introspective observation. Every accentuation lengthens the sound upon which it falls, and every composition of an auditory series into a rhythmical unit transforms the apparent magnitude of the intervals which separate it from preceding and following groups. These expansions and contractions of duration vary in extent with the intensity of the sounds which support them, and in virtue of this constant functional relation they uniformly escape notice.

The process of rhythmical integration must therefore be taken into account as an important factor in every consideration of the phenomena of the time-sense and the means by which we apprehend and estimate series of successive impressions. Since our discrimination of durations is always of intevals bounded by limiting sensations, there is made possible the appearance

of a process of rhythmical accompaniment wherever certain temporal conditions obtain among the successive stimuli of the series. If the latter thus originate a rhythmical mode of apprehension there will at once be introduced specific errors of estimation which will transform the whole nature of the intervals to be compared. The process of sensory rhythmization must therefore be considered first in its functional relation to general nature of the time-sense, and, if it be an indispensable factor in the estimation of temporal quantities, the effects of its various specific forms must further be discriminated in the analysis of the conditions under which appreciation of time-relations takes place, and of the factors which make for accuracy and inaccuracy in the process.

The relations of rhythmization to the time-sense are inevitably misconstrued by the observer who depends upon introspection for his results, and to this source is to be attributed the error of those psychologists who describe the sense of time as attaining its greatest refinement in the apprehension of rhythmical series. Thus Sully says, (Outlines of Psychology, Pt. III, p. 172, Am. Ed.). "It is, however, in the ryhthmic successions of verse and music that the ear's appreciation of time-relations shows itself at its best. The essential element in this experience is regular recurrence after a definite interval, or periodicity. Here an accurate measurement of time-interval becomes essential." The results of experimental observation contradict such an idea. There is present in all musical and poetical perception a characteristic transformation of time-values which makes the immediate comparison of successive intervals impossible. The estimation of absolute duration in rhythm depends finally upon factors which are themselves subject to variation. It is not the accurate measurement of successive intervals of time which is essential in rhythm, but the maintenance of proportionate relations among successive groups.

This preservation of time-analogies depends upon the capacity for a very nice reproduction of a succession of experiences of strain. Accurate estimation of time in music obtains only between intervals whose durations measure like strain experiences. Intervals bounded in the same way, or having similar functions in the musical sequence, are perceived and reproduced with great accuracy indeed; but no dynamically unlike intervals can ever be justly estimated as time-extents. Change the position of the given interval in the series of which it forms a part, alter its function in the rhythm group, increase or decrease the accentuation which its limiting sensations receive, and its psychological duration is immediately transformed beyond recognition. The same absolute interval has as many time-values

as there are varying structural relations in the rhythmical form of the music or verse in which it appears.

It is just when the succession of intervals is not part of a rhythmical sequence that accurate comparison of their values becomes possible. To this end each interval must have an independent existence; the moment two become members of a common structural unit the possibility of immediately comparing their extents disappears. The latter process is strictly one of automization, and characterizes all perception and production of rhythmical quantities. I think Meumann has seized only half the truth when he says that an estimation of the time of movement in beating verse exists so long as the movements are not automized; and that when automized the periods are maintained without being estimated. The former relation is a condition which is never realized. So long as the successive beats and intervals are not automized,—so long, that is, as the successive intervals do not coalesce into groups which themselves replace the single beats as units, no rhythmic perception arises, and no scansion or beating of time is possible. The effect of rhythmization in thus masking the absolute values of the time intervals involved was noted by Mach in 1866. He remarks (Untersuch. u. d. Zeitsinn d. Ohres, p. 181) that when iambic series are frequently repeated in the same direction the hearer ceases to discriminate the longer from the shorter interval. Similarly Hall and Jastrow write in their Studies of Rhythm (Mind, Vol. XI, p. 61) that "after the series had been heard two or even three times, no impression of the relative length of the middle interval (of three) would often exist, and only after hearing the fourth and last would the judgment incline to the plus or minus side." The work of practically every experimentalist since these early observations were made has incorporated the same results.

The critical comparison of duration in successive groups of elements in a rhythmical sequence likewise disappears as an immediate experience. Such groups occupy objectively the same time-extents only when,—and because—they have the same functional relations in the verse and the stanza. The moment a higher synthesis appears and two formally like groups assume relations of super- and sub-ordination to one another,—as when one receives a major, the other a minor accent in a common higher group—the perception of their absolute time-values becomes impossible. On the other hand, as in the case of primary rhythmical quantities, the attempt to observe the larger structures of the passage and the stanza, for the purpose of comparing their durations, is instantly destructive of the rhythmical impression.

It is practically beyond question that every motor accompani-

ment of a series of regularly recurrent sensations tends to interfere with the proper estimation of the time-values of their intervals by thus becoming automatic. Müller and Schumann report that they have found such estimations most accurate when the limiting sensations are passively listened to; and that the ability to make exact comparisons is interfered with when motor activity is introduced. Ebhardt ascribes this interference to a displacement of the function of judgment by the process of motor activity which occupies the period in which the comparison must take place. The displacement of discrimination by another process is important in proportion as the motor adaptation makes large demands upon the process of attention, but it plays a diminishing part as the systems of reactions become more and more automized. It is a familiar observation that mechanized series of this kind may exist without any perceptible drain upon attention, and it seems difficult, at first blush, to account for the interference which their establishment arouses.

Automization of any process means characteristically an increased regularity and stability in its relations; while the process controlled by selective consciousness is as typically marked by variability in its form and instability in its internal relations. The intervals between automized movements are certainly more uniform than those between reactions which are consciously supervised. If the process involved only the automization of a series of concomitant movements synchronous with the series of sensations, and the perception of departures from uniformity in the occurrence of this succession, then we should expect the process of time-estimation to be facilitated instead of obstructed by the introduction of the factor of movement.

The interference, however, is not to be doubted. Its source is to be looked for in either (or both) of two factors. The process of automization of movement takes place under certain laws of natural rhythm which are characteristic of the individual but which probably differ but slightly from subject to subject. Thus Stevens, in a series of experiments on the time-sense (Mind, Vol. XI, p. 393), required his subjects to accompany the beats of a metronome by synchronous movements, and to continue the series at the same rate after the instrument had been stopped. The two series agreed only when intervals of a certain magnitude occurred between adjacent beats. In the one case there was coincidence, in the other interference with the natural period of the subjective rhythm, which in these experiments varied from 53σ to 87σ. In Scripture's investigation "the intervals chosen were, on an average, 92, 94, 152, 156, 160, and 180σ." If the intervals to be compared are in-

commensurable with the period of this bodily rhythm, the lack of correspondence between their phases exerts a disturbing instead of a facilitating effect upon the process of time estimation. Münsterberg, in his work on the comparative estimation of successive durations, has emphasized the fact that there is no function of consciousness which can be called a special time-sense. Subjective standards of measurement are uniformly dependent upon physiologically conditioned changes, in sense-organ, muscle, respiration and the like. For periods of short duration rhythmical variations in the tension of the sense-organs afford the basis of our judgments; in the immediate apprehension of longer periods our estimates are chiefly determined by the functional rhythms of respiration. Rise and fall of tension, forming a completed wave of sensation, accompany each act of inspiration and expiration. According as the impressions coincide or fail to coincide with like phases of this process, the duration of their separating intervals will be judged clearly or confusedly; and according as they endure through equal or unequal multiples of this unit will they be judged to be equivalent or non-equivalent.

But secondly, all beating of time, whether or not consciously intended to embody a specific rhythm form, tends to fall into secondary rhythms by which the objective values of the intervals to be compared are destroyed as elements of presentation. Neither the centrally determined series of reactions nor that which seeks to reproduce an absolutely uniform succession of sensory impressions is free from rhythmic phases. The types of differentiation which such systems of reactions present affect both the intensity of its elements and their temporal relations. The definition and extent of such free rhythmization depend upon the absolute rate of succession among the stimuli involved, upon the quality of these stimuli, and upon the nature of the synchronous attention process. Its occurrence makes the process of time-comparison difficult, insecure and in any strict sense impossible. Attention must be renewed at every act if the series is not thus to be automized and fall into accentual groups. The moment inattention ensues, or the series is taken as a whole or in groups, the opposing phases of rhythm appear. Either, therefore, the process of motor adjustment requires attention and the judgment is displaced, or it is automized and the material of judgment is transformed in value. In both cases the accurate discrimination of duration will be seriously interfered with.

The phenomena of mechanized motor impulses seems thus to be essentially antagonistic to the process of time-discrimination. On the contrary, it is upon the phases of such rhythmical processes, in the last analysis, that not only forms of æsthetic

apprehension but the sense of time itself depends. Contradiction appears and the capacity for correct discrimination is destroyed only when the intervals to be compared are bounded by dynamically unlike limits. The reproduction through a motor process of the intervals to be compared, which is spontaneously established in the presence of exciting stimuli, is indispensable to the function of temporal judgment. Horwicz calls rhythm the only measure of time, and ventures the assertion that a being whose experience did not manifest periodicity could gain no conception of its nature. Time does not make rhythm, but rhythm time.

Such phases of generally rhythmical change may belong to either of two categories, namely sensation and movement; and theorists of the time-sense have severally based their derivation of its phenomena upon each of these. Thus Lipps connects our judgments of time with the process of fading which the course of sensations manifests, qualitative differences in the elements of consciousness being translated into concepts of temporal order and value. But such experiences are qualitatively rhythmical only, and give no adequate basis for time comparison; for the series of external stimulations is without any sufficient principle of periodicity, to some form of which the phenomena of the time-sense must finally be referred.

Within the organism itself such rhythmical changes are abundant, and to these facts the majority of observers have turned for an explanation of temporal judgments. There are certain constant conditions of feeling, as Waitz points out, which characteristically accompany these processes of change in sensation or other conscious content,—straining of the attention, feelings of expectation and surprise, of impatience and tediousness,—and it is to these, not to the disorderly palimpsest of fading sensations, that we most hopefully look for an understanding of the facts of time-estimation. "This theory of temporal signs," writes George C. Robertson (referring to Dr. Ward's view of movements of attention as temporal signs), "may fairly be regarded as the most interesting contribution to the subject since Herbart."

Wundt likewise connects the process of accurate time-estimation with primitive types of rhythm in the bodily activities. He points out the fact that the interval of time of which we have the clearest grasp as an object of sensible intuition, judged by the degree of sensitiveness to variations in its extent, is just that which Weber had observed to be the average time occupied by the leg-swing in rapid walking. We can estimate such periods best because the laws of our physical organism make it most easy to mark time to them by some form of organic adjustment or motor reaction. The value of this period is

variously given by different experimental observers, as follows: Mach, 0.375 sec.; Stevens, 0.71 sec.; Kollert, 0.755 sec.; Mehner, 2.15 secs.; Vierordt, 3-3.5 secs. Of these only two (0.71 and 0.755) present even a moderate approximation to each other; but through the whole series runs a fairly defined periodicity. The unit of this rhythm is not the value ordinarily assigned to the period of most accurate estimation,—three-quarters of a second—but about one-half of this. All the subsequent members of the series, if we include Vierordt's results, which present rather the indifference-point at which the tendencies to over- and under-estimation disappear, embody this primary period. It seems probable, therefore, that the duration most accurately estimated is one including both phases of a complete rhythmical change.

As to the special processess of adjustment involved in the apprehension of temporal quantities there is probably no last word to be said, in so far as the phenomena of time-estimation are not necessarily correlated with any one type of organic change. Horwicz considers that all the rhythmical functions of the body serve this purpose in turn,—respiration, pulse, movements of the legs, jaws, etc., and the alternations in larger rhythms of hunger, sleep, work and the like. The immediate apprehension of time is, however, to be understood only in connection with rhythmical changes in the organism of imcomparably shorter period than those of sleep and waking, labor and rest. Of these shorter rhythms most stress has been laid upon respiration and the beating of the heart. Leumann compared the natural rates of reading verse in different persons with the normal periods of their pulses, and found that one with a pulse of eighty-five beats per minute read one hundred and seven metrical feet in a minute, while one with a pulse of ninety-eight read at the rate of one hundred and twenty-nine feet. He found also that the pulse of a single subject changed from seventy-seven, when scanning one hundred and thirteen feet per minute, to eighty-three when scanning one hundred and forty feet. With changes of this type Leumann connects the phenomena of attention waves, and what James calls "the rhythmical sharpening of our time-sense."

Mach considers the time-sense to be as special as that of vision. He surmises that its organ is to be found in the mechanism of the ear; that processes of accommodation are set up by sensations, varying with their intensity, duration, etc.; and that from the phases of such adaptation has originated the apprehension of succession and duration, analogous with the perception of distance and perspective by the eye. It is questionable if the ear is an analogue of the eye in this regard. The elements of visual space-perception are derivative in their

nature, and represent certain primary successions of motor sensations involved in the exploration of the external world. But this set of secondary perceptions is exceedingly well organized and independent. In various animal types it presents a system of sensori-motor adaptations organized in advance of individual experience and depending upon the inherited assimilative formula of the organism itself. In human perception all pronounced motor interpretation has fallen into abeyance and we depend in a very complete fashion upon immediate visual sensation for our representation of space-relations.

If we assume the ear to be capable of analogously becoming the organ of time-perception, it is still only as a secondary mechanism the changes in which are representative of certain primary experiences of tension and release. Such substitutionary agencies arise only in virtue of the greater sensibility or control of special organs, and development by no means exists here in any such degree as characterizes visual space perception. Introspection reveals the existence at every point of wide-spread rhythmical tensions and releases in the organism, as the accompaniment of all attentive time-estimation. The inhibition of these, or the contravention of their phases, means the mutilation of our time-perception. Whatever may be the possible future development of auditory function in the apprehension of time-values, the process is still intimately bound up with these primary sensations of tension and movement themselves.

In so far as the element of organic adjustment is concerned in the appearance of time-values it must be recognized that this process cannot be localized in a single mechanism, such as the ear, without presupposing an evolution for which there is little empirical justification. The estimation of time is in no way restricted to auditory experience; one judges as readily,—if not so accurately—the length of flashes in a coast-wise light as the duration of tones in a siren. The qualitative content is indifferent, and the variety of organic adjustments is as great as the number of senses which may be specifically involved. The essential point of the matter lies, not in the quality of the sensations or character of mechanism involved, but in the phenomena of general attentive adjustment common to all these processes alike. The root of the matter is reached by Titchener when he says: "When we try to discover by introspection what means we have used for our comparison of two durations of this third kind (one-half to three seconds), we find that strain intensities have played a great part in the formation of the judgment. The strain sensations come 1, from the expectant attitude of the whole body, and 2, from the adjustment of the sense-organ to the stimuli which limit the intervals to be compared. We estimate duration in terms of intensity: the more intensive the

strain, the longer must the interval have been; the less the strain, the shorter the time."[1]

The process of rhythmic integration is fundamentally involved not only in the estimation of relative duration in time intervals, but also in the numerical apprehension of serial impressions beyond very simple groups. Our immediate apprehension of number in temporal series is exceedingly weak, and extends at most to groups of half a dozen members. If the rate of succession be sufficiently low counting may be employed, and there is then no limit to the number of impressions of which one may keep tally; but this is in no sense the apprehension of a series. Each impression is perceived and named separately, and the process involves, in addition to this, only a memorizing of the final name uttered,—an ordinal term which becomes a symbol for the sum of the whole series.

The apprehension of a serial group is wholly unlike this. We can grasp only those series of impressions which are either so simple or so highly co-ordinated as to have a distinct form for our consciousness. Simple groups which are so characterized extend to four or five, possibly half a dozen individuals. Beyond this we fall back upon the process of rhythmization, the result of which is to extend such figuration to larger and larger series. We identify twenty-five impressions as we identify five; when the series is increased beyond the limits of apprehension as a group of individual elements we can estimate its number only when the conditions are such as to allow of the substitution of groups for elements as units in the process of apperception. What we recognize is not a larger number of units but a like number of units of a qualitatively different kind. They are such as we call "three-beat," or "five-beat," or "two-fours-beat" groups. The identification of the unit-group in such a case is as immediate and secure as the recognition of simple beats, or elements of any kind. The naming of the whole series as "ten," or "twenty-five" or "forty" strokes, is a secondary interpretation of the significance of the immediate apprehension. The form of the "twenty-experience" or the "forty-experience" is recognized as is that of any rhythmical sequence, and subsequent analysis alone translates that qualitative series into numerical values. Without this rhythmical emphasis and grouping such estimation would be absolutely impossible. The scope of this temporal consciousness depends upon the complexity of the units which enter into it. "Forty strokes might (then) be remembered as a whole, and identified without error when repeated, provided the mind grasped them in five sub-groups of eight, or in eight sub-groups of five strokes

[1]An Outline of Psychology, Cap. IV, p. 87.

each. When no grouping of the strokes beyond making *couples* of them by the attention was allowed—and practically it was found impossible not to group them in at least this simplest of all ways—sixteen was the largest number that could be clearly apprehended as a whole."[1] The particular number-groups apprehended with the greatest ease are those most readily subdivisible into rhythmical units; the most difficult are the prime numbers. "Series of 4, 6, 8, 16 were more easily identified than series of 10, 12, 14, 18. The latter could hardly be clearly grasped at all. Among odd numbers, 3, 5, 7, were the series easiest caught; next, 9, 15; hardest of all, 11 and 13; and 17 was impossible to apprehend."[2]

Such auditory series cannot be grasped at all if their succession falls below a certain rate, for beyond this limit the human consciousness has no capacity for rhythmical integrations of its impressions. When their succession rises above a certain other superior limit the numerical apprehension of the series of impressions again becomes confused, for while the rhythmic form still persists, the number of elements which are crowded into each simple group can no longer be discriminated. The range of rates within which such rhythmic integration is manifested in combination with clear discrimination of constituent sounds lies between two seconds and one-tenth of a second. The most favorable rate appears to be slightly more rapid than that instinctively adopted in free rhythmical tapping. This is due, I believe, to a difference in the mechanism of accompaniment in the two cases. The muscle system employed in beating out a rhythm with the finger presents a greater inertia than is involved in accompanying a sensory series by motor discharges which affect only certain parts of the vocal apparatus.

The limits of our capacity for estimating temporally extended periods or numerical series are to be looked for in the physiological laws which condition motor discharges on the one side, and make it possible or impossible for us to imitate the objective series by a system of organic strains; and on the other hand, in the limits placed upon our discrimination of refined experiences of strain due to perception-reflexes taking place in some part of the bodily organism.

[1] Dietze, summed by James: Psychology, I, Cap. XV, p. 613.
[2] *Op. cit.*, p. 613.

MENTAL LIFE OF TWO *MACACUS RHESUS* MONKEYS IN CAPTIVITY.—I.

By A. J. KINNAMAN, Fellow in Clark University.

INTRODUCTION.

To Dr. Edward L. Thorndike, of Columbia, belongs the honor of having first taken the monkey into the psychological laboratory. He experimented for several months with three *Cebus* monkeys from South America. His methods and work have been made a starting point for the following studies. In my experiments two *Macacus rhesus* monkeys from India have been used. This species is peculiar in that it is a connecting link between monkeys and baboons. It is a favorite among trainers, and is regarded as very intelligent, though not so intelligent as the *Cebus*. No extensive studies have yet been made of it from the standpoint of psychology. Cuvier, however, gives a charming story of a mother *Macacus rhesus* and her young. Not having access to the original report, I have translated it from Brehm, and the translation will be found in the appendix to this paper. So far as I know his is the only careful study yet made of any of the psychical qualities of the *rhesus*, though there are extant many scraps of information concerning them of more or less scientific value.

I wish here to thank President G. Stanley Hall for suggesting the investigation and for valuable assistance in procuring the literature of the subject. I owe acknowledgment to Dr. E. C. Sanford, likewise, for numerous suggestions, and assistance in procuring and arranging of apparatus, and for help and criticism both in the experiments and in the evaluation of them. I wish also to thank Mr. Wilson, the Librarian of the University, for assistance rendered in the library, Dr. Hodge for the use of his fine camera, and many others for valuable assistance given.

METHODS OF COMPARATIVE PSYCHOLOGY.

METHODS OF SECURING DATA.

Kline regarded the methods of studying animal psychology as of two sorts: (1) the natural method, consisting in careful and continuous observations of the free life of an animal. In this group belong Huber, Moggridge and McCook on ants,

Audubon on birds, Figuier on insects, and Mill on our domestic animals; and (2) the experimental method, which consists in putting a question, and subjecting the animal to such conditions as will favor the performance of activities that shall contribute material for answering the question asked. While the classification is a correct one I desire to divide still further, and to indicate five methods of studying the characteristics and capacities of animals. Each has its advocates, and valuable results have been obtained by all of them. Some render only qualitative data, others both qualitative and quantitative.

(1) *Free observation of animals in their native habitat.* Disciples of this plan are to be found in Ernest Seton Thompson, W. J. Long, Paul du Chaillu, John Burroughs, Lubbock, Audubon and Forel. The first four of these are literary. While they attempt careful observation of the animals, they do not hesitate to read into them all sorts of feelings and mental capacities, surpassed only by the fertile imagination of Uncle Remus. The method from a literary standpoint fully commends itself, but such studies must not be mistaken for science. If the method is to add anything to the stock of human knowledge the literary observer must take the greatest care to keep the foundation of his work entirely within the field of truth. But adding to the sum of human knowledge is not the only outcome of study worthy of attainment. The literary observers are adding a rich fund to the sum of human sympathy and interest, and that is enough so long as in making that addition fundamentally false conceptions of the real animal life are not inculcated.

But from a purely scientific standpoint also, this method can be made to bear first-class fruit. It has the advantage of seeing the real animal in his natural, unhampered reactions. Lubbock and Forel studied the smaller animals and insects in this way. It is an easy matter to bring whole colonies of these under direct observation and yet to leave them much of their native freedom. The larger animals cannot well be studied by this method. It can hardly, save for the lowest forms and smaller animals, ever be more than qualitative in its results, though Lubbock has demonstrated that for ants, bees and wasps the method can be made to yield both qualitative and quantitative results.

(2) *The second method* is ably presented by Wesley Mills in his *Animal Intelligence*, p. 7. A single quotation from him with reference to the study of a dog will give a fair idea of the method which he proposes and uses.

"Not only is it necessary in order to understand the individual dog to begin with him at his birth and to follow his history throughout, but such a course is essential for the com-

prehension of the nature of dogs in general, and, personally, I am deeply convinced of the importance of such investigations, after having been engaged in them for some years." In other words the young animal is to be studied rather than the adult, but the study is to comprehend his gradual development as well as his reactions in his natural environment. "When such studies are carried out on representatives of different groups of animals, and on different breeds or individuals, one's conception of the true nature of animal intelligence—or, to use a more comprehensive term, the psychic life of animals—is vastly widened and altogether more correct in every respect."

The method is invaluable in a study of instincts, individual differences, growth, development, and the influences of environment. No other methods can hope for results more significant for genetic psychology and education. The stages of development can here be seen to pass along from germ to maturity in very rapid succession. Yet attempts to carry its results over into the realm of human activities are always to be guarded with great care. If students of animals are always in danger of reading their own processes, unduly, into the animals that they study, those that apply the results of such studies to human conditions are similarly liable to read animal conditions into the psychic life of man. It will be recalled that Comenius, probably more than any one else, in this way, made nature the criterion of methods in a somewhat characteristic pedagogical scheme.

However valuable this method, it has its limitations. While it brings out what the young animal *does* it omits both what it and the adult *can do*. Only a limited variety of animals can be so studied at all, and a still more limited number, while reacting unhampered by human hand in their relation to their natural parents. The method has been successfully applied in studies of chicks, kittens, puppies and squirrels.

(3) *Training*. This method is presented by P. Hachet-Souplet in his *Examen Psychologique des Animaux*. He proposes to study the animal by training it, holding that rational training multiplies for the animals, especially for the superior animals, that is to say the most interesting ones, the occasions for bringing into play, before the observer, their intellectual and instinctive faculties, enabling him not only to arrive at significant conclusions but easily to verify them. The animal is to be brought by repetition to such a stage that he shall know by a verbal order or by a gesture from the trainer just what he is to do. Thus we have the animal studied in action. The method reveals but little beyond the powers of memory and association. Hachet-Souplet proposes that animals be classified on the basis of psychic capacities and that suscepti-

bility to training be taken as the proper criterion for determining mental capabilities. On this basis he makes three classes of animals. (1) Those capable of being persuaded to perform their parts. Such are said to act chiefly through intelligence. (2) Those which cannot be persuaded but must be coerced. These are said to act through instinct. (3) Those in which excitation only is possible. They possess, evidently, neither intelligence nor instinct, they are simply excitable and live without psychic direction, only by repetition of physical and chemical phenomena, which determine and continue their nutrition and reproduction. If memory and association were the only signs of intelligence this scheme for the classification of the psychic faculties would be complete though very general. To say the least the scheme is ingenious and deserves serious consideration.

(4) *Free observation of animals in captivity.* This method was employed by Cuvier, Romanes and Garner in their studies of monkeys. Probably no other method suffers so great limitations. The caged animal often ceases to be himself. He varies from insipid tameness and moroseness to wild excitement, and is in several ways very different from what he is in his larger freedom. The method is in reality but an application of the first method described above to a single case of the one yet to be described. It is a free observation of an animal's reaction to a single kind of apparatus—the cage. Those employing the method, for the most part, have observed loosely and have interpreted rather liberally. The method is only semi-scientific, and the results that have been obtained have not always been very reliable. Such expressions as "A great many times," "A great many monkeys," etc., are too loose for scientific use. Studies on this plan have ascribed to the animals observed all sorts of capacities. In addition to loose observation, Mr. Romanes has unfortunately given credence to many stories by hunters and other untrained persons where the details of the stories were wholly omitted. Very often when one has the full situation the wonderful achievements of the animal drop to the level of the common-place. The reader will find such an illustration in my report of the monkey's reaction to the trees, given in the next section. Besides the fact of imperfect observation the ordinary observer is prompted by an inordinate desire to exalt the powers of his "pet dog Solomon," and make him appear as wonderful as possible. But when put to careful test the wisdom of "Solomon" is often found to fall below the level of that of the ordinary cur. Among the students who have ascribed undue value to these current stories of animal intelligence, the most prominent are Houzeau, Lind-

say, Romanes and Weir. The great Darwin himself has not wholly escaped this tendency.

We would commend this method as a partial one, but like other methods, it needs the checks and control tests of other methods to keep it in the straight and narrow way. I count it a poor method but one by no means to be wholly abandoned.

(5) *Experimental.* This method consists essentially in subjecting the animal to fixed or controllable conditions, and in noting its qualitative and quantitative reactions. Lloyd Morgan, Kline, Small, Thorndike, and others, as is well known, have employed this method somewhat extensively. By way of example, it may be well to present here Thorndike's explanation of the method, when dogs and cats were the subjects.

"By this method of experimentation the animals are put in situations which call into activity their mental functions and permit them to be carefully observed. One may, by following it, observe personally more intelligent acts than are included in any anecdotal collection. And this actual vision of animals in the act of using their minds is far more fruitful than any amount of histories of what animals have done without the history of how they did it. But besides offering this opportunity for purposeful and systematic observation, our method is valuable because it frees the animal from any influence of the observer. The animal's behavior is independent of any factors save its own hunger, the mechanism of the box it is in, the food outside, and such general matters as fatigue, indisposition, etc. Therefore the work done by one investigator may be repeated and verified or modified by another. No personal factor is present save in the observation and interpretation. Again, our method gives some very important results which are quite uninfluenced by any personal factor in any way. The curves showing the progress of the formation of associations, which are obtained from the records of the times taken by the animals in successive trials, are facts which can be obtained by any observer who can tell time. They are absolute, and whatever can be deduced from them is sure. So also the question of whether an animal does or does not form a certain association requires for an answer no higher qualification in the observer than a pair of eyes. The literature of animal psychology shows so uniformly and often so sadly the influence of the personal equation that any method which can partially eliminate it deserves a trial."

If the trials are continued for some time one has a large element of training, and the method comes to have a great deal in common with method four. With this method is afforded opportunity to study association, memory, imagina-

tion the senses, powers of generalization and reasoning, besides individual differences and educability. We get here, as in the method of training, not so much what the animal *does do* in his native environment as what he is *capable of doing* under somewhat artificial conditions. It alone can furnish opportunity for anything like exact quantitative work, while it possesses all the advantages of training so far as repetitions and critical observations are concerned. Its limitation is to be found chiefly in this, that it loses the native activities of the animals and does not use the signs ordinarily employed in training by persuasion without the stimulus of food.

One of the great difficulties in the employment of this method is that of selecting tests and apparatus properly related to the character of the animal studied. Taking only such activities as are wholly natural to the animal, and such apparatus as is easiest for him to operate, makes his reactions very simple, and the test fails to bring out his possibilities. On the other hand, the tests selected must not be too far removed from the field of his natural activity, for in such cases the animal is often completely baffled, and only the most limited results are obtainable. The animal may appear stupid because the test is unsuitable, and thus be easily misjudged.

The highest possible knowledge of an animal is hardly to be reached by the employment of any one of these methods. The methods are not equally applicable to all animals. Yet there is every reason for believing that if all are employed upon the same animal, as far as possible, we shall arrive at a more complete understanding of him. Not opposition to one method or undue emphasis upon another, but a reasonable use of each within the just limits of its applicability, should be the practice of the student of comparative psychology.

Methods of Interpreting Data.

Wundt sets forth two points of view in animal psychology.[1] "We may set out from the notion of a kind of comparative physiology of mind, a universal history of the development of the mental life in the organic world. Then the observation of animals is the more important matter; man is only considered as one, though of course the highest, of the developmental stages to be examined. Or we may make human psychology the principal object of investigation. Then the expression of mental life in animals will be taken into account only so far as they throw light upon the evolution of consciousness in man." Wundt pursues the second of these courses, and insists that "not the least advance can be made, either in the psychology

[1] Human and Animal Psychology, p. 340.

of a particular animal or in that of the animal kingdom without starting out from the facts of the human consciousness." In spite of all the warnings against reading our own powers and processes into the lower animals, when we undertake to study their psychic phenomena, I am not certain but that that very thing is necessary, within limits, in all fruitful studies of the mental life of animals. But the careful student of human and animal minds will avoid prematurely concluding that because he finds the mental factors and processes thus and so in one of these great fields of investigation, they necessarily obtain in the other in the same relation. The schematized evolutional life of animals possibly applies to man. But direct and unhesitating application should be questioned.

Again, the camp of comparative psychologists divides on the application of the *Lex parsimoniae* in the interpretation of their data. There can be no doubt but that the application of the law is a virtue and its rejection a vice. In practice, there are on the one hand extremists who ascribe to animals, with an almost pitiful blindness, and without limit, all sorts of human and angelic powers; on the other hand, there are other extremists who reduce every mental process of the animal to a common level in lowest terms—tropisms, instincts and associations. The former are virtually vicious and the latter are victims of their virtue. In the one camp we find steeped more or less in the vice, Romanes, Weir, Garner, and Mills; in the other, Wundt, Morgan, Thorndike and Loeb.

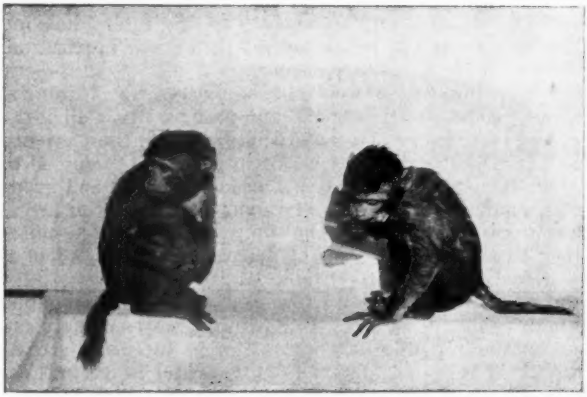
As in many other cases, the true practice is in all probability to be found between that of the extremists. My sympathies lie nearer to the "victims of the virtue;" nevertheless, it seems to me that we must not concede that the mental processes of the animal are so unlike human processes as to be absolutely simple.

If in any class of animals one can be shown to imitate, remember, invent or reason, then a complex reaction of any one of the class cannot safely be explained without reference to this possible process as a part of the complex. Over-simplicity is the rock of danger to the over-parsimonious.

General Observations.

My first monkey, a male *Macacus rhesus*, arrived Oct. 15. The female came about one month later. The male was said to be about eight months old, and the female, twelve. Jack is more nervous than his mate, Jill. His nervous mechanism is set off by little noises and threatenings which pass entirely unnoticed by Jill. Shipping had greatly frightened him. He sprang about the cage, threatening, and uttering a loud call by ones, twos, threes and fours. This call or scream is hard to

represent by letters or the human voice, but it is approached by *hwuich*. It appeared to be prompted by a feeling of loneliness, lostness, strangeness and hunger, and to be given as a call to learn the whereabouts of others of his kind. As a caged animal, his social instincts do not appear to be very highly developed, though in its natural habitat the species plays and goes foraging in bands. The calls were most vigorous on the first and second days and almost wholly disappeared in about a week. While experimenting he has given the call a few times with apparently no other stimulus than hunger. The frequency of these calls can be judged from two ten-minute records: Oct. 17, 21 times; Oct. 18, 18 times.



JILL. JACK.

When a live frog was thrown into the cage, he grabbed it but let it go immediately and sprang to his perch. He appeared to be taken by surprise, and was not really afraid of it, for he returned immediately, caught it up and pulled it to pieces using both hands and teeth in the process. He did not appear to do it as if destroying an enemy, but as a mere feelingless business matter, as if it were something that ought to be done and he did it. If he had any feeling about it, it did not betray itself. His face, like that of a baboon, for indeed he is very baboon-like, is almost totally expressionless. To me, it has as yet but two forms: one with the mouth closed and one with the mouth opened. The latter occurs when he crouches and threatens or tries to bluff one, and when he sees food and apparently thinks how good it would taste. I believe

that much of the monkey's expressions of triumph, villainy, jealousy, anger and risibility are either in the manner and speed of limb and body movement, or in the viscera of the observers who read into them a feeling like that which they suppose they would have under similar circumstances.

These monkeys show fright by hasty springs in the cage, or by running and looking back when loose in the large room. When baffled in an attempt to open a box for food Jack walked away uttering a mild audible sigh, *hweuwh*. Surprise is shown by a quick jerk of the body; anger, on the part of the male, at the collar, by quick jerky efforts to bite or tear it, and curiosity, by gentle peeps and lifting an object up with extended hand. He did this with a rattle box put into the cage; and, again with a paper sack containing a small snake, of which he was afraid, yet, curiosity led even to tearing a peep-hole in the bottom of the sack, through which, with much caution, he often looked at his hereditary enemy.

The gnawing, which was considerable at first and continues to some extent, appears to be prompted by the instinct of escape, and leads him to lay hold wherever there is good gnawing. Some of the gnawing was done about the door. I think it is safe to assume that if gnawing at any point had brought release, gnawing afterwards would have appeared to be much more purposive.

They have shown no signs of play in any way. They take life seriously. When I first saw Jack springing up and down on the pan in the bottom of the cage I was half inclined to take it as a play manifestation. But he afterwards did the same thing on the top of a box into which he was failing to make his way. It appeared to be a sort of useless effort at just doing anything to get into the box or get out of the cage.

When a newt, which looks something like a snake, was put into the cage the monkey showed no fear of it. He did not tear it to pieces, as he did the frog, but rolled it between his palms many times, and at last bit it through the head and threw it down.

Jack and a pigeon were equally frightened at each other when in the cage together. Yet at times he would swing down from the wire or perch as if to touch or grab it, but at the same time revealed his fear of it.

A full grown cat brought into the room showed no fear of the monkeys whatever, but they were desperately afraid of it. They produced the "*nqu-u-w*" sounds in profusion, sprang into the windows, and sought the farthest corners of the room, even engaging in a mutual spat for vantage places of safety. On the following day when they were turned out of their cages

they climbed into the windows and looked about everywhere for the cat.

I had been told that Dr. Thorndike kept his monkeys from touching things by putting a ball of cotton batting on them. Accordingly I covered a pear almost completely with cotton, pinning it fast with pieces of toothpicks and pulling the cotton out in a fluffy ball. I threw it into the cage. Showing no sign of fear whatever, he tore away the cotton with hands and teeth, and ate and pulled alternately until the pear was completely eaten.

On Oct. 20, I tied a rope to the end of Jack's little chain, and fastened the rope to a large post in the center of the room. After many accidents I succeeded in getting him properly tethered. He ran across his circular area of action receiving many jerks. But he profited by this and learned, except when scared, to avoid the jerking by carrying a foot of slack in his chain as he ran about or walked erect. He often paced thus along an arc of his circle until some twenty feet from his cage when he turned and ran back erect. But being frightened he broke the hook at the collar and gained his larger freedom. This freedom he has retained. When I waved a stick toward him he ran back to the cage. When the door of the cage is opened he usually goes in at once of his own accord. At a motion of my hand or at my calling "Get down, Jack," he gets down from a case or out of a window, especially if he sees that I am certain to approach if he does not do it. If I pretend to be at work and order him down in an ordinary tone of voice, without changing emphasis or inflection, he pays no attention. The monkeys have given no sign that they know their names.

The sounds produced by the male are the loud *hwuich* and the softer, *hweuwh*, an expulsion of breath through mouth and nostrils, a blowing of air out between the loosely held lips, and a grinding or a gritting of the teeth. These last seem to have no well defined purpose. All my efforts to reproduce these vocalizations have utterly failed to call out any response from them. The nostril and mouth expulsion I presume is the one represented by Mr. Garner by the formula, *nqu-u-w*. It is uttered most frequently when food is displayed and while eating, but may accompany disturbance by the keeper or by visitors. The sounds of the female are a loud call, corresponding to the male's *hwuich*, resembling the call of the woodpecker; a softer sound closely resembling the mew of a cat; the *nqu-u-w;* blowing between the loosely held lips; a baboon-like growl uttered when I kicked violently at her as if to strike her, and a high pitched screech.

In the early part of their larger freedom they spent much time sitting in the windows. They appeared interested in the

street cars and wagons that passed along the street at the other side of the campus and enjoyed the warm sunshine. Later, I cut and set up three small trees in the room. They showed no interest in the trees until they began to bear bits of fruit and bread. Here their instinctive skill and ingenuity in getting food from the most inaccessible tips of branches was of especial interest. They seemed to scan the limbs and plan long before reaching the limb where food had been placed, whether to bend the limb around to a stronger one or to follow it to the end and drop to the floor or swing back to the limb and pass down the trunk. When food was placed on a strong limb that extended several feet to one side and pressed against the ceiling, Jack climbed part way up, then backed out and went to an adjoining limb where he seemed to examine the situation in order to determine the best line of attack. After examining from two places he went up the limb to a part near the ceiling then swung under and went hand over hand until the food was reached. He grabbed the fruit, let go with his hands and crammed the food into his cheek pouches while hanging by his feet, head downward. This operation completed he returned to the floor. At one time food was put on the end of a stick which was fastened to a gas jet. The stick projected horizontally out toward the middle of the room. The food was so high that the monkey could not spring up to it. One of the trees stood near. He ascended a long slender limb fully five feet away from the apple. His weight bent the limb down to the apple and he won the prize. The over-zealous for reasoning in animals can here be sure that the monkey showed a comprehension of a mathematical principle and of the law of gravity. But such was not the case. He sat at the farther end of the room while the food was being arranged. From his position it would be difficult to tell just how far it was from the limb to the apple. Besides this the monkey had been fed often from the end of this limb. Seeing food arranged near there he merely followed the old track, and as he did so found himself gradually coming nearer and nearer to the goal. There was no reason about it, and certainly no comprehension of mathematical and physical principles.

After a few weeks the trees were used considerably as perches to which the monkeys often retreated between tests, especially if the rearranging of the apparatus required some time. The window perches have come to be used only occasionally.

When food was shown the male it will be remembered that he dropped his chin and opened his mouth. Under the same provocation the female worked her lips rapidly. Some interpret this as a sign of anger, bluff or assault. Mr. Garner thinks that it may be "intended as a vote of thanks." In my opinion the *rhesus* never gives thanks for anything. He is not of the

thankful sort. Nor yet does it express anger. It seems to me to be a kind of impatient nervous overflow intermingled with a notion of the taste and the eating activities to follow, while waiting for the food finally to be put where it can be reached, and is stimulated immediately by the sight of the food itself. The child represents the same physical excess by movements of the head, arms and legs, while he cries, "One for the money, two for the show, three to make ready and four to go." Jill has spent more time in the trees and less in the windows than Jack, shows less fright and is generally slower in all of her movements. No snakes, frogs or pigeons have been put into her cage.

The two have shown no spirit of play either separately or when together. A wheel like that used by squirrels was arranged, but it was never used except to secure food, and as a vantage point for seeing what was going on.

When out in the room, if food is thrown to them, both rush for it, but when either gets it the other respects the right of possession.

Sign language is used and understood somewhat. I have already spoken of the recognition of signs when delivered by the keeper. Twice the female struck with her hand when she thought the male was too near her food. If either is master it is she. When together, their favorite pastime is "flea hunting" which is really dust and dandruff hunting. Backing up in front of the mate, lying down or assuming a sort of pose is understood to mean "pick me." However, this sign language, if indeed it can in any sense be regarded as such, is only of the most rudimentary sort.

In general I may say that so far as I can learn, their reactions are in many ways like those of their wild state, and they show the same kind of ingenuity in meeting and solving their problems, in procuring food, in escaping from an enemy, and in finding their associates.

Repetition of Dr. Thorndike's Experiments.

Relation to Dr. Thorndike's Work.

Dr. Thorndike experimented with two *Cebus* monkeys from South America. He used a great variety of apparatus, consisting largely of designs with which food was associated, and of boxes having doors fastened with various kinds of latches. In these were placed food which the monkeys could get by moving the button or other device holding the door shut. I began my work by repeating, in the main, the experiments which he made. I have used in all thirteen kinds of fastenings for the doors of the boxes. Nine of these were afterwards put

into second positions on the doors. Besides this seven groups of these fastenings were used. Three string devices also and a windlass were used in connection with the cage. Opportunity was afforded for but few experiments on imitation, and none by putting the animal through a process to be learned, since this species of monkey, like most wild animals, objects to being touched or handled, unless one begins with them when they are very young. While they have crowded by me for food or pulled my hands open for it, I have yet for the first time to touch them actively. The memory tests have been made with apparatus not used by Dr. Thorndike, so I reserve a discussion of them for another part of the paper. Only a few pieces of my apparatus are identical with Dr. Thorndike's, but the general character of them is the same.

General Conditions.

Those who have seen monkeys in captivity and in their native habitats have noted in some of them great differences under the two conditions. The caged gorilla is sullen; the female chimpanzee is often morose and sits about in a drooping position or engages in swinging the body to and fro. I have seen the golden baboon, when not entertained, indulge in a swaying of the body which was a cross between a swing and a vertical movement. Most monkeys, if they age in captivity, become crabbed and treacherous, however affectionate when young. I am not aware just to what extent the *rhesus* shares in this change of character but it is certain that his ill disposition does not disappear.

It is safe to assume that all studies of animals in cages and pens would show variations from those of animals in their native haunts. Such studies with the *rhesus* are less unnatural than with South American monkeys, for example, since owing to the peculiar attitude of the natives of India toward monkeys, they have become bold enough to take up their abode quite in the seats of civilization. At home they are in a degree reverenced. They may be chased from fields, gardens and orchards, and may be shipped by steam car or caravan into distant regions, or trained and sold for a foreign market, but they must not be killed. These monkeys enter the villages in troops to beg and steal food at the stores. They are acquainted therefore with man and his ways and works, buildings, streets, etc. Hence, a rather free captivity such as this pair has had (they were loose in a very large room), does not subject them to a wholly new environment.

In experimenting, the monkeys were kept hungry enough to make them keen in their efforts to procure food. It can hardly be maintained that they were subjected by this to unusual

conditions, since in their wild state, it is no uncommon thing for them to become ravenously hungry. Starvation is always a menace and a not very remote possibility to all wild animals.

It has been urged that the tests to which monkeys as well as other animals are subjected by our fifth method, described in a previous section, are foreign to their apperceptive stock of ideas: that such experimenting is somewhat like asking a biologist or anthropologist to solve a difficult problem in higher mathematics, and by it the animals are put at a great disadvantage. They may appear to lack mental capacities, it is contended, simply because the tests employed are outside of their normal experiences and instincts. The criticism, so far as the Bhunder monkey is concerned, is only partly just. In his woody home he is engaged much of his time in searching for food. He must meet various situations in order to get fruit from the tips of slender limbs. Bark must be pulled away to get a fallen nut or to catch an insect. Fallen limbs and sticks must be removed in order to procure a nut or a root. These acts are not entirely unlike moving a bolt, button or hook, or pulling a string or plug. If these are new to him and he manages to operate them, then the whole matter becomes highly favorable to the mental capacity of the monkey.

Just how much these limitations have affected the natural reactions of the monkeys it is quite impossible to determine. It is safe to say that such tests as have been made do not give an unduly favorable view of their capacities. In any event I present here the results obtained under the conditions described, however favorable or unfavorable they may have been.

Apparatus.

The food was put into a box 11x13x15 in., having a door 6x8 in. in the middle of one side of it, and having hinges below so that the door opened outward and downward. A spring set on the inside of the door caused it to fly open as soon as released.

(The numbers on the cut, p. 112, correspond to the numbers of the fastenings described below. Only a part of the fastenings are shown.)

A. *Simple fastenings.* 1. A thumb-button extending in front of the upper right-hand corner of the door. It must be turned upwards or downwards about thirty degrees to release the door.

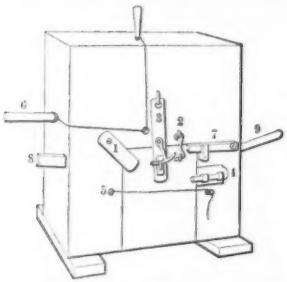
2. A small hook fastened to the box above the upper right-hand corner of the door and hooking into a staple below. This will be referred to as the vertical hook.

3. The T-latch. It was a thin plate of iron fastened by a staple above the middle of the door. By means of a slot in the

lower end of the plate it could be hooked over a staple through which was passed one end of a small T attached to the plate. The T came in from the right.

4. Bolt. This was an ordinary door-bolt. Pushing the bolt back allowed the door to open.

5. String. The string was fastened to a short screw to the left of the door, and wound eight times around a nail at the right. The door could be opened by unwinding the string.



6. Plug. A string fastened to the inside of the door passed upward an inch and thence through a hole in the front of the box, and around to the end where it was fastened to a plug. The plug was stuck into a hole in the box. The plug being pulled out, released the string and the door flew open.

7. Lift-latch. A bar having one end fastened to the box by means of a screw extended behind a cleat near the door, while the other end extended over the upper right-hand corner of the door. Raising the end of the bar next to the door two inches released the door.

8. The push-bar. A bar two inches wide and one-half inch thick passed into the end of the box just back of the boards on the front side and extended clear across just behind the upper part of the door. The cleat of (7) was put on the inside of the door and a notch corresponding to it cut in the underneath side of the pusher. By closing the door and drawing the pusher back three-quarters of an inch the door was held fast. When the pusher was thrust back into the box the end of the cleat passed through the notch and the door flew open.

9. Bear-down latch. It was arranged like the pusher except that it had to be pushed downward about two inches to effect a release.

10. String and bolt. A common door bolt at the top of the door and on the inside of the box could be raised by pulling a string at the rear of the box.

11. String and ring. This was the same as five except that

instead of winding the string around the nail a ring hooked over it.

12. Horizontal hook. It was the same as the vertical hook except that it held the horizontal position.

13. Lock and key. The lock was the ordinary door lock. It was placed on the inside of the box. The spring was removed from it that it might require only a minimum of force to turn the key.

B. *Chute-apparatus.* This consisted of a chute five feet long and inclined at about fifty degrees. At the upper end there was a pivoted tilting-board which being tilted allowed a morsel of food to slide down the chute. The wire front of the cage was brought near the lower end of the chute.

14. A string extended from the lower part of the tilting-board into the lower part of the cage, ending in a toggle that kept the end of the string within the cage. By pulling the string, food could be started down the chute. The monkey could reach out from the cage and get it.

15. The string passed from the tilting-board over a pulley 12 inches above the rear part of the top of the cage. The end of the string then passed down through a hole into the cage. By going to the back part of the cage and pulling the string the tilting-board was tipped and the food could be procured as in 14.

16. The string passed down from the tilting-board to the axle of a windlass. The handle of the windlass was in easy reach from the cage. By turning one and one-half times around the board was tilted, and food obtained as before.

C. 17. A string four feet long was tied at one end to a flat paper box. The other end was fastened to a toggle inside of the lower front part of the cage. By pulling the string in food could be procured.

D. 18. Imitation test. The lock and key. Problem—to put the key into the lock, turn it and procure food.

E. *Second positions.* When any suitable form of fastening was first used on one side of the door it was afterwards changed to the other side. These need only to be named as they have already been described in their first positions. They are: 19. Button. 20. Vertical hook. 21. T-latch. 22. Bolt. 23. String and nail. 24. Plug. 25. Lift-latch. 26. Push-bar. 27. Bear-down latch.

F. *Groups of fastenings.* Again these have been described individually and need but to be named. These might be released in any order. 28. Two buttons. 29. Two bolts. 30. Button and bolt. 31. Two buttons and one bolt. 32. Two buttons and two bolts. 33. Two plugs. 34. Three plugs, one being on the top of the box, two buttons and two bolts.

G. *Designs.* The apparatus consisted of two tin cans, each 2x1-2x2 1-2x3 inches. They were covered inside and outside with white paper, and so arranged that a marking card could be slipped in close to one side, leaving its top extending about three inches above the top of the can. On the side of the card facing the top of the box designs were drawn. The problem was to associate food with the box bearing the design.

35. The card on one box had four heavy, black horizontal lines, each one-eighth of an inch wide, two inches long and separated by blank spaces each one-fourth of an inch wide. The other card was blank.

36. A diamond one inch long and three-fourths of an inch wide on one card, the other blank.

37. A box like the one used with the fastenings, but with the back removed. This was placed with the door facing and near the front of the cage. The door of the small box was fastened with the thumb-button. Two large pieces of cardboard, one having a black paper 4x4 inches on the middle of one side were used as signs. In experimenting, providing food was placed behind the door of the small box, the card with the black square was held before the door of that box, between it and the cage, until well seen. It was then moved and the monkey was allowed, if he desired to do so, to move the button and take the food. When no food was placed there, a blank card was used in the same way. A table for the order of feeding was made out previously. The problem was to see whether the monkey would learn to open the door when the card with the black spot was shown, and not to do so when the plain card was shown.

Method of Work.

In experimenting, except with the string and windlass devices, the monkey was allowed the freedom of the large room. When a series of tests were to be made a few grains of rice or a small morsel of bread, banana or apple was put into the box and the door closed and fastened. The monkey was allowed to see this operation, but was never allowed to see me open the door except for an imitation test. By giving quite small baits the experimenting could continue indefinitely without producing satiety. When he lagged in his work or was inclined to sit in the window or to give the fastening up as beyond a monkey's ken, he could be spurred by showing a banana and tapping it against the door of the box, especially if by sleight of hand it was made to disappear from view at that point. Sometimes he was spurred by showing him the morsel in the box, and again by showing him a whole pile of food in the box. He would apparently work harder for much than for little, quite as if he had his price.

A cumulative stop-watch was used, and account was taken only of the time actually spent in trying to open the box. Time was counted no matter whether the monkey was before or behind the box, whether prancing around it or jumping up and down on top of it, so long as he was trying to open it. Some of these efforts were in no wise directed toward the latch. At first he did not appear even to suspect that the latch was a proper point of attack. But this prancing, etc., was, doubtless, effort to him. Much time was spent in shaking the box when the lock was a difficult one. For this reason the box was nailed fast to the floor.

The male only was taken regularly through these tests. The female gave up so quickly and completely with each new fastening, that to get her to move the bar, lock or button it was necessary to half conceal rice about it so that in the effort to procure the food the lock would be moved. Having repeated this several times she would come to associate the moving of the fastening with the opening of the door. Her time results, therefore, are not to be compared with his. Her work with the locks will be described from time to time under appropriate headings.

Only one fastening was given up by the male. That was the horizontal hook when first presented. He had four hours at his command, and spent seventeen minutes in prancing, shaking, gnawing and jumping up and down. This was the first thing presented to him. We shall revert to it later for a further discussion.

After the above each lock and group of locks was presented and he was allowed to work until the door was opened whether this time was long or short. When the door was opened and the morsel of food eaten, the trap was set again, and so on for thirty times, the seconds required for opening the door each time being recorded.

Results.

The following table and curves indicate the quantitative results of the tests with the male.

This table shows the times in seconds required for working the different fastenings including also the chute, and string and box (apparatus 17) tests. The figures designate the number of seconds used in working the device for each one of the thirty times.

The variations in the times for the fastenings are very great. While on the average the second effort required one-fourth as much time as the first, the T-latch required only 1-462. Again it required nearly twice as long to draw the plug the second time as it did the first time. It is plain that the button, bolt, bear-

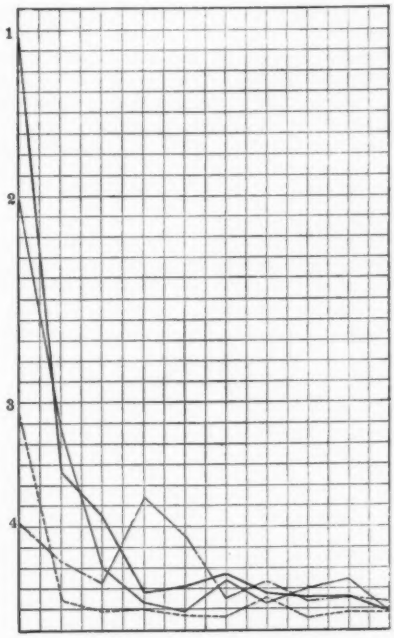
Button	10	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1
Vert. Hook	65	62	3	10	2	40	30	40	67	10	1	25	1	1	59	3	3	9	3	1	20	24	1	1	1	1	1	1	1	1	2
T-Latch	462	1	1	8	6	52	8	31	1	6	2	12	13	1	3	3	8	3	1	2	4	1	1	2	1	1	1	4	4	4	
Bolt	1	5	1	1	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2	1	1	1	1	2	1	1
String & Nail	80	5	3	6	7	7	6	7	12	6	4	9	3	75	36	76	7	6	9	29	28	7	12	14	30	14	23	27	8	8	
Plug	125	266	114	30	14	2	3	1	13	2	3	1	1	5	5	2	3	2	5	3	3	3	1	1	1	1	1	1	1	1	
Lift-Latch	15	81	12	4	5	3	2	7	7	7	2	2	3	5	2	2	2	4	2	2	1	2	2	2	1	3	2	1	1	3	
Push-Bar	168	5	1	3	4	1	3	6	2	1	4	7	1	7	1	9	2	1	1	1	1	1	1	1	2	1	1	1	1	1	
Rear-Down	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	2	1	1	2	1	
Rear-String	46	6	1	4	3	2	3	1	1	1	1	1	1	1	1	1	2	3	1	5	3	1	3	1	3	1	1	1	1	1	
Ring & Nail	213	15	22	9	7	14	47	8	5	7	3	7	1	5	1	1	1	2	1	1	1	1	1	1	1	1	1	1	1	1	
String-Box	98	57	4	5	4	7	6	5	5	7	8	3	4	5	6	5	4	10	7	6	4	4	5	5	3	4	6	4	3	4	
Hor. Hook	32	4	9	2	2	1	1	1	3	2	1	1	4	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	
Chute Str. below	3	3	2	2	5	3	6	1	3	1	2	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	
Chute Str. back	366	6	218	18	4	79	7	2	3	1	10	1	1	5	2	1	2	2	2	4	1	1	1	1	2	2	1	1	1	1	
Windlass	672	127	21	16	6	5	12	10	4	9	10	3	4	2	2	14	2	9	2	2	2	8	7	9	18	3	3	3	7	6	
Lock & Key	100	9	54	32	104	5	6	3	3	3	3	9	1	1	8	1	2	1	2	2	1	1	1	1	2	1	1	1	1	1	
Aver.	80.6	38.5	27.5	9	10.3	13.2	8.5	7.4	7.7	3.9	3.1	4.6	2.5	6.9	7.7	7.3	2.5	3.5	2.4	8.7	4.4	3.5	2.5	2.6	4.1	2.3	2.8	3.1	2.1	2.2	

down latch and chute with string below, were the easiest for him. Some, as the vertical hook and T-latch, were hard to learn, but easy to manipulate when once learned. Others, as the string-nail, string-box and windlass were difficult to learn, and were of a type that required considerable time for manipulation even though well learned.

The results of the first ten tests with all the different fastenings are shown in Curve 1. The seconds are shown on the ordinate by half millimeters. This curve is not, of course, a final representation of the general curve of learning by trial, as it is based upon only seventeen cases (seventeen kinds of locks). However it corresponds well with the curves found by others, and is doubtless of the same general character as one that might be obtained from several hundred cases with the same apparatus. One sees here the rapid decrease of time at the first. The minimum of time is practically reached at the tenth trial, as the table shows. With the individual locks there were considerable relapses at times after the tenth trial. The average time, however, soon becomes measurably regular. The frequent record of single seconds in the table are to be understood as indicating that the animal went to the fastening and released it, with his characteristic speed, with a single movement. The time was often less than one second.

Second Positions of the Locks.

Nine of these locks were put on the door in first and second positions, that is, on the right and left sides of it. The averages of the times required for moving the fastenings, when in



first positions, are given here in the upper line of figures. The averages for the second positions appear in the lower line.

114.1	47.5	15.2	7.2	4.6	12	6.2	10.5	11.7	3.9	2.1
52	7.4	4.8	5.8	4.5	3.2	7.4	2.5	3.8	4.2	3.4
5.7	2.7	10.7	12.1	19.9	3.1	3.2	2.7	4.5	6.6	4.5
2.5	6.3	6.3	6.4	2.8	3.3	2.5	6.1	3.8	2.4	1.7
2.4	2.7	4.3	2.7	3.5	4.3	2.2	2.3	(See Curve 2).		
1.9	3.2	3.1	3.5	4.0	3.5	2.1	1.7	(See Curve 3).		

Curves 2 and 3 show respectively the relative times for working these nine fastenings (see apparatus 19-27) in the first and second positions for the first ten trials. On the average about one-half as much time was consumed for second positions as for first positions. Here the problem seems to have been to learn that the lock was in a new place and that it was necessary to work it in a direction opposite to that used in its former position. It is significant that on the average when these nine locks were in the first position the second manipulation re-

quired nearly one-half as much time as the first manipulation required; while when in their second positions the second manipulation required only a little more than one-seventh as much time as the first one did. While the new position and direction were very troublesome, probably because they stood in direct opposition to the previous associations, when these were once overcome so many remained common elements that their interference was limited to the first trial. After seven trials the time was usually as short as it had been in the thirtieth trial in the first position. It may be assumed that the learning of the new position and direction requires the excess time in the first seven trials over that of the eighth. In that case the mastering of the new position and direction required, roughly speaking, 50—5—2—3—2—1—5 seconds. It is clearly more difficult for the monkey to learn a new device than to master the same device in a new place.

Groups of Locks.

After the fastenings had been learned singly the seven groupings described under apparatus (28-34) were made. In these we find an initial drop in time for the second test of but two-thirds of the first time. This drop is much less than that for single openings. The average time for moving the group of locks the first time is, however, much less than the average time required for learning the single locks. The minimum time is practically reached in the sixth instead of the tenth trial. The averages for the groups are 25.5, 16.5, 11, 31.5, 12.9, 7.3, 11.1, 6.4, 7.4, 6, 9.6, 8.3, 10.6, 9.3, 4.4, 5.7, 6.3, 5.7, 3.7, 7.0, 7.9, 12.7, 6.4, 5.1, 7.4, 5.1, 9.4, 10.1, 5.1, 6.0.

Curve 4 shows these results graphically for the first ten tests. The great rise in the curve for the fourth trial was due to a confusion over the group of seven fastenings. In these cases the method of opening each part was already known; the problem was to get to all the parts and to know what ones had not yet been moved. The time could not be reduced so much as when single fastenings were used, since it was necessary for the monkey to go to different parts of the box in order to reach the several locks. When the group consisted of two or three fastenings the monkey soon adopted a regular routine which he rarely failed to follow.

Experiments with Designs.

Three hundred tests were made with apparatus 37. It was assumed that if the monkey learned to associate the card having the black center with the securing of food and the blank card with not securing food, he would turn the button when

the former was presented, and would not do it when the latter was presented.

Although each card was presented 150 times no association was formed and the button was moved each time without a moment's delay throughout the 300 tests.

Apparatus 35 was constructed next. The food was put into the box where the card bore the four horizontal bars. The two boxes were then set down in front of the monkey, about ten inches apart. After he took the food, a new morsel was slipped in, sometimes from one hand and sometimes from the other. The boxes were then picked up, passed behind my back and exchanged or not exchanged and then returned to the floor facing the new position of the animal. In these he chose the right box 154 times and the wrong one 146 times. Clearly there was no association of the food with this design. The tests were certainly extensive enough, as will be seen when we examine the results where glasses covered with paper of different shades were substituted for the boxes. The monkey failed most likely because his attention was never caught by the marks upon the card. The blank card on the box not having food placed in it, was cut away leaving a strip projecting upwards about three inches. The other box was surmounted with a large square card. Three hundred-eighty tests were made as before of which 242 were right and 138 wrong. Afterwards two vertical strips each about one inch wide and three inches long, one colored blue and the other green, were used with 38 right cases to 52 wrong ones. Glasses were now substituted for the boxes. One glass was covered with black paper and the other with white. Food was put in the one covered with black. The result was that he chose in the following order, + standing for right choice and — for wrong: + — + — + — — + + — +. Though many more presentations were made, there were no more wrong choices. Here we have the association fixed very readily when the apparatus was such as really appealed to the animal.

With the female 50 tests were made with the boxes. One of the cards was blank while the other bore a large black diamond. Thirty of the choices were wrong and only twenty right. But again when paper covered glasses were substituted we have: — + + — — + + + — — + — + + + + + + + + + +. There having been no errors in the last ten choices I considered the association perfected. With considerable justice, it can be inferred from the experiments with the covered glasses that the association with the preceding designs, if it was ever to have been formed, would have appeared with the opportunity given.

Comparison of the Results of the Present Experiments with those Obtained by Dr. Thorndike.

In these experiments, as in Dr. Thorndike's, there appeared no case that could be interpreted as reasoning in the higher senses of that term. When the box was set for the male the first time he went to it and pushed rapidly and violently right and left up and down around the door. Where the edge of the door projected a little beyond the level of the box it was both gnawed and clawed. After working there awhile he chased around the box and went at it again. Then the box was shaken and attacked again. The edges and corners of the box were jerked and gnawed. This was continued for seventy seconds, when he stopped and engaged in a short "flea-hunt." This was followed with alternate work and rest throughout a period of four hours, seventeen minutes having been used in actual effort. In all this time the horizontal hook had hardly so much as been discovered, and apparently there was no notion that it had anything to do with preventing the door from opening. The female tried this hook similarly, two months later, but could not be induced to do more than look at the box after 102 seconds of trial. After the male had entirely given up, the hook was removed and the button was then put on. He opened the box now in ten seconds, but the opening was a mere accident of the general scramble. However, the table will show that he profited by his fortunate accident. In moving it the second time he did not deliberately put his hand on the lock. He seemed to think that scrambling at about a certain place would get the door open, and so he scrambled there. After the eighth trial he put his hand every time directly on the latch and moved it. Throughout the experiments we find what Dr. Thorndike designated as "gradual learning by a gradual elimination of unsuccessful movements and a gradual reinforcement of the successful ones." In this way, however, effort came to be fairly well directed even on attacking a new lock.

Special movements with "directness (which reminds one unavoidably of human actions guided by ideas") appeared in the course of the monkey's efforts; for example, in the application of the teeth where the hands failed, and again in the substitution of hands for teeth where a fastener had been learned and moved several times previously with the teeth, but might be moved more readily with the hands. Dr. Thorndike noted substitutions of this kind with the *Cebus* monkeys. Some of the lower animals under similar circumstances never make substitutions but follow persistently the first accidental method and association. Presumably the animal which can make such a

substitution has a psychic life superior to the one which cannot do it.

Methods of Learning.

(1) *Learning by trial.* I have already mentioned learning by trial more or less definitely directed, fortunate accidents, recollection of these acts, elimination of the useless efforts, substitution of more appropriate methods of accomplishing the end in view.

(2) *Learning by imitation.* Only one opportunity was made use of for getting an imitation of an act performed by the experimenter, and in that case it failed. After the door of the box had been opened as many as thirty times with the key so arranged that it could not be removed from the lock, it was removed and placed near the door. The monkey did not so much as seem to discover that the key was the object that he had just been using. It was picked up both with the hands and with the feet apparently as a mere accident. I then picked it up and opened the door with it fifty different times with the monkey only two feet away and looking on. There was absolutely no effort at imitation. Imitation of one animal by the other, however, was more successful. I tried on four consecutive days to teach the female to work apparatus (6). In order to get her food she had to pull a plug out of the end of the box. After 5½ minutes of trying she gave it up, hardly so much as discovering the plug. But she gnawed the string in two several times. Rice was then put into the hole around the plug, and even out on the plug itself. In the course of an hour I succeeded in this way in having the plug drawn seven times, but only once without rice. She seized the plug with her teeth near where it entered into the box. It was chewed and pulled right and left until it finally fell out. Except in one instance the drawing of the plug did not in the least seem to be associated with the opening of the door. On three other days the experiment was repeated without putting food on the plug. It was pulled right and left but was never drawn. Further, she became so thoroughly discouraged that she would walk away from the box without effort as soon as she saw that the plug was the mode of fastening, even though she had seen rice and other food put into it. At this juncture the male was turned out of his cage. He went immediately to the box, she following some four feet away. Knowing the trick perfectly he seized the end of the plug with his teeth and removed it. I set the box again. This time the female rushed to it, seized the plug by the end as the male did, and procured the food. This she repeated immediately eight times in exactly the same way. On succeeding

days she removed the plug as a part of a combination lock on the same plan 130 times.

Recalling that she had failed to work the bear-down lever for opening the box (see apparatus list No. 9), I placed it before her. She rushed up, but missing the plug she sat down. The male passed her, pushed the lever down and procured the food. When the box was set again she worked the lever and took the food in the same way that he had done. She manipulated this apparatus several times immediately and 250 times later as a part of a combination lock. Besides these, once when the male peeped under the bottom of one of the trees the female came and peeped in the same manner. Neither obtained any food, and neither reached under for any. There have been numerous cases where one followed the other when it jumped onto a table or into a window. But such cases of imitation are rather doubtful. It seems to me that the two cases with the box are quite as good examples of imitation as could well be gotten even with human beings. While this is an unusual method of learning on the part of the *rhesus*, the above example seems to me conclusive evidence that it is at least a possible method for him.

(3) *By singling out special elements in a complex perception.* In this there is progress by the singling out of definite points from the general hazy whole that must have impressed the monkeys in their earlier attacks. The horizontal hook at first was no definite part of the whole box, but after all the other fastenings had been worked by them, including the vertical hook in two positions, the horizontal hook was replaced. Now the vagueness had been reduced. The hook, like the other later fastenings, was singled out and attacked directly. Through a great many scratchy movements at it it was moved by the male in 32 seconds as against 1,000 seconds at the beginning which had ended in failure. The female on returning to the horizontal hook moved it in 5 seconds as against 102 seconds with failure at the beginning of her tests. It appears that there was improvement in dealing with fastenings in general. All locks toward the last were attacked with definiteness and as if the animals had a dim realization that they were the proper objects for attack, a thing which was not true at first. This growing capacity to deal at once and with considerable success with a new lock was very noticeable. It is an index of the monkeys' educability and of the road by which they progress. It looks very much like the possession of a sort of general notion fairly well represented by projecting-thing-has-something-to-do-with-it, and so they attacked the projecting thing and not something else. (This and some similar cases will be analyzed in the latter part of this paper.) This is evidence of

educability within narrow limits. If one accepts intellectual evolution he might expect, at least, as a possibility with the higher classes of animals, a small degree of general improvement.

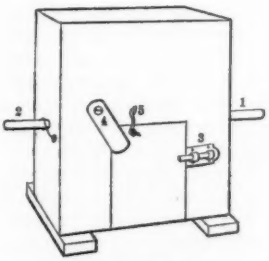
To sum up briefly this phase of my experimental work I find:

1. The monkeys have not reasoned in the higher sense of the term.
2. First efforts require much more time than later ones, the time shortening very quickly. There are frequent reverses after an association has been fairly well fixed.
3. The monkeys are capable of substituting a better for a poorer method of manipulation.
4. In dealing with groups of manipulations they adopt a regular order.
5. The main body of their learning has been by trial and happy accidents, the recollection of these and the elimination of useless efforts.
6. The female, however, has learned by imitation.
7. They have shown an increasing ability to pick out the fastenings as the essential point of difficulty in opening the box, and to direct their efforts upon them alone.

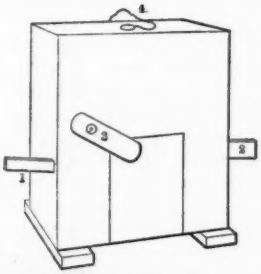
Combination Locks.

The remainder of this paper is based on experiments which, so far as I am aware, have never before been made on monkeys. The first have to do with locks of a more complicated character.

Apparatus.



Combination I.



Combination II.

1. A box like that used in the previous experiments was again employed. The door was fastened by a vertical hook at the upper left-hand corner so arranged that a button near it had to be pushed back before the hook could be moved. But

the button could not be moved until the bolt at the right side of the door was pushed back. And this in turn required the removal of a plug at the left end of the box before it could be moved, and finally a plug at the right side of the box had to be moved first of all. So the only order of removal which could result in success was, right plug, left plug, bolt, button, hook. In the cut this order is indicated by the numbers, 1, 2, 3, 4, 5. Further, this was the only order in which they could be moved.

2. The same box. The fastenings this time, and their order are, bear-down lever at the left, push-in bar at the right, lift-up latch in front, and pull-out string behind. These are all so arranged that when once moved they lock and cannot be shaken back and thus prevent moving the next part of the combination. The cut indicates the plan, and the figures the order, as with combination lock 1.

Method.

Lock 1 was used first. The hook was in reality no additional element since when the hand moved the button the thumb caught the hook, so that except in rare instances, the hook caused no additional movement whatever. When experimenting with the female the hook was left off. Each of the parts of the combination lock had been moved thirty times as a single lock before it was drawn into the combination. The problem was, therefore, not that of learning the movement of the parts but of learning the order of movements which would get the food. Feeding, timing, etc., was conducted as before. In addition an attendant recorded the time and made the notes dictated so that my whole time could be devoted to the observations. The record of the monkeys' movements was kept by calling off the number of the part of the combination attacked, and of the second at which any part was moved. When starting out I set the arbitrary standard that the combination would be considered as learned whenever it was worked ten times consecutively without error. Every effort to move any part out of its regular order was considered as an error. With combination lock 1, 253 trials by the male and 80 by the female were required to meet this arbitrary standard. In evaluating the results the last three tests of the 253 above were omitted. After this each monkey was worked 250 times with the second lock. Neither attained the arbitrary standard with it.

The following tables of times and errors indicate the progress quantitatively. For convenience the 250 tests are thrown into 25 consecutive groups of ten each and the *average time* for each of these groups is given. The errors have been treated in the same way.

Table of Times.

First Combination Lock.	Male	34.0	28.3	33.1	14.4	9.5	9.0	9.8	11.3	10.6	8.4	11.9	8.8	9.9	11.4	8.9	9.2	7.4	6.2	11.4	5.9	7.2	7.8	6.1	5.6	6.4
	Female	89.8	44.	31.	16.2	17.5	10.1	6.1	5.1																	
Second Combination Lock.	Male	105.3	80.3	68.7	31.0	40.8	27.0	20.0	22.7	22.0	21.0	15.0	9.4	21.5	22.5	14.3	19.5	17.4	17.7	12.5	12.1	12.9	13.9	13.0	12.4	13.6
	Female	100.1	63.2	61.2	77.8	32.5	39.4	16.2	20.6	19.6	23.7	20.1	27.8	23.5	21.3	20.4	22.6	23.3	19.4	18.1	17.7	18.2	20.7	18.7	12.0	19.6

Table of Errors.

First Combination Lock.	Male	6.1	5.5	8.4	4.1	2.4	2.3	2.3	3.6	3.5	2.2	3.2	2.1	2.5	3.2	2.2	2.2	1.6	.8	1.9	.7	1.3	1.3	1.4	.7	.4
	Female	11.9	11.8	7.6	3.8	4.1	1.2	.3	.0																	
Second Combination Lock.	Male	26.8	16.5	12.6	7.4	8.8	9.5	6.5	11.4	8.3	7.3	4.0	1.3	6.8	8.3	5.3	6.1	7.5	9.1	4.4	3.9	4.7	5.1	5.3	4.7	5.0
	Female	18.2	12.8	13.8	23.3	12.1	15.7	7.8	7.3	7.8	9.9	9.5	13.3	11.6	10.3	9.4	10.3	9.9	10.8	9.4	9.9	9.3	11.4	9.0	4.3	7.4

The tables show both in times and errors a rather rapid reduction at first and a slower reduction later, along with numerous relapses. It will be noticed that on the second combination lock the twelfth group of 10 tests with the male shows a very great reduction both in time and errors, immediately followed by higher numbers and that this low standard was not reached again. At this twelfth group 7 out of 10 of the efforts were without error. I expected him to reach the standard in the next group, but at this juncture a distinguished friend and visitor came in for a few minutes. The animal took fright, and as it was very late, the experimenting was postponed to the following day with the result above. While the break prevented the monkey from accomplishing his feat it furnished a fine example of the effect of the fright and the fifteen hours' delay on the slow mastery of what was proving to be a very difficult task.

It will be seen that the female reached our arbitrary standard with the first lock in eighty tests. A further comparison of the processes of the male and female here as in the case of many of the other tests will be made later under the heading of "Individual Differences."

In working combination 1 the male went from the right plug by way of the front of the box to the left plug. In doing so he passed by the bolt and button, neither of which could be moved before the left plug was removed. In the earlier attempts he tried these fastenings as he passed by them, later he would raise his hand as if to put it on the bolt, and then as if remembering that it could do no good he would withdraw the hand without touching. This was repeated several times before he came to pass it by without notice.

The female went from the right plug around by way of the back of the box, but for a long time tried the bolt, using the left hand, before starting around. Here again came a course of ten or twelve cases in which the hand was extended part way and then as if to say "no, that doesn't come next," she withdrew the hand and went on her way.

Twelve human adults and five children were tested with the same combination lock. Unfortunately no record was kept of the number of their errors. Later, two adults and two children were systematically tested with both combinations. The first and second times, given in seconds for the twelve adults were, 90-5, 45-4, 69-8, 13-3, 11-3, 57-7, 19-4, 300-6, 154-5, 132-7, 20-4 and 7-3. Correspondingly the times for the children were, 112-30, 216-21, 184-31, 65-13 and 45-7. The monkeys' first two times were, 78-33 and 64-65. The two adults tested ten times with combination 1 reduced their times respectively from 6 and 8 seconds to 1½ and 2 seconds. The

children reduced their times respectively from 80 and 45 seconds to 2 and 2 seconds. The monkeys' show no material change from their first to their tenth trials. Read as above their times are, 78 and 54 for the first and 52 and 90 for their tenth trials. With the second combination the two adults changed from 11 and 36 to 2 and 4. The children changed from 105 and 20 to 6 and 3, while the monkeys changed from 139 and 187 to 110 and 26.

Allowing due range for individual differences it yet appears that the results obtained with the children resemble the results obtained with the monkeys more than do those obtained with adults. The essential method of learning these combinations on the part of human beings is just the same as on the part of the monkeys, that is, trial, the remembrance of accidental successes and the dropping away of superfluous efforts. The nature of the locks gives little opportunity for reasoning one's way to success.

When the human adult attacks the box he looks at it and attempts to reason about it. If he tries the wrong latch first he is slow about attacking it again. He usually tries to move a latch by the use of strength enough to break it. If urged not to break it, he stops and tries to reason again. When he gets one latch moved and takes hold of a wrong one next he is liable to return and try to replace the one already moved. But when once through he usually remembers the road to success and retraces it very rapidly. By the third time he has practically reached his highest speed.

The child tries immediately, and, like the adult, plays havoc with the apparatus if it is not equal to his strength. He holds on, where he tries first, for some seconds. Then he looks bewildered, tries something else as before, draws a long breath and looks helplessly to you for directions. Urged to go ahead he may try the same thing over. Finally he succeeds in moving one part. He stops and looks up, apparently with a feeling that it came too easy or as if he feared that he had broken the machine. Errors persist longer with him than with the adult, and his time is reduced more gradually.

But when the monkey attacks the box he does not stop to think about it. He tries one thing, then another and another in quick succession. One part worked he tries all the others, though he may labor on one at times, and try often to move parts that have already been moved. If he fails to move the first part of the combination he may go again and again over the second, third and fourth parts. As he does so he comes to make no real effort at any one of them. Each is given only a passing trial or a mere touch.

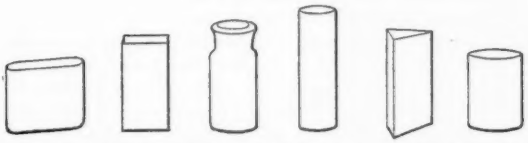
The short-circuiting process by which speed and skill are

finally attained is of great interest. Take for example, a record of the male's second effort to work the second combination. The order for success should be (1), (2), (3), (4). The following series of figures shows the course of the monkey's efforts. His successes are shown in heavy faced type when he got it right. **1,** 3, 2, 4, 3, 2, 3, 2, 4, 2, 1, 2, 3, 1, 3, 2, 1, 3, 4, 1, 3, 2, 4, 1, 4, 2, 3, 3, 2, 3, 1, 4, 2, 1, 3, 2, 4, 1, 3, 3, 2, 4, 1, 3, 2, 3, 1, 4, 2, 1, 4, 2, 1, 4, 2, 3, 2, 1, 4, 1, 3, 4, 2, 4, 1, 3, 4, 4, 3, 4, 3, 2, 4, 1, 3, **2,** 1, **3, 4.** Here we have 75 errors. It will be seen that latch (2) was tried 19 times before it was moved. The latch moved easily enough, but he either pushed it in the wrong direction, or made no real effort at it. The above is a record of his second trial and is one of his longest. The twentieth trial reads, 2, 4, 2, 3, **1,** 3, 4, **2,** 3, 4, **3, 4.** This latter is a typical case for more than a hundred trials. Later, between the 110th and the 120th trials, he made no mistake in 7 out of 10 times that he opened it. In the first trials he seems to have forgotten how to move (2), which was the push-bar. When that was moved he usually finished very quickly. But take a case like the last one quoted above, how is the monkey to know that the extra pulling is not a part of the game, since he can see no immediate advantage in any of the movements except the last one? Yet he does manage to get the usless efforts out. Take his 86th trial. It reads, 2, **1, 2,** 4, **3, 4.** Here he has improved over the 20th trial by dropping out the useless 2, 3, and 4 before 1; the useless 3 and 4 before 2 and one useless 3 before 3. This short-circuiting is not a rational process. It, like most other cases of his learning, is accomplished by the method of trials. In his fumbling and prancing he went at odd times from 1 to 2 without stopping at 3 or going around to 4. This then brought the result without the intervention of useless trials. Gradually, then, these successes were associated until at last they were firmly fixed in their correct relation. The order 3-4 was the first part learned. The order 1-2 came second, and the order 2-3 was learned last.

Summary. In the course of this group of experiments we have met two excellent cases of inhibition on the part of the monkeys. We have found that they are able to deal with complex and difficult pieces of apparatus; and that their learning by trial and happy accidents is not limited to the learning of single and simple devices. Lastly, the experiments have furnished us an opportunity to compare animals and human beings in their reaction to the same apparatus under much the same conditions. I believe that a series of experiments with this and similar complex apparatus could be made on children and monkeys under conditions quite the same, and would furnish mate-

rial for one of the most interesting chapters in comparative psychology.

FORM TESTS.



In order to test the monkeys' ability to discriminate forms, I arranged six small vessels each with a capacity of half a pint. These consisted of a wide mouthed bottle, a small cylindrical glass, an elliptical tin box, a triangular paper box, a rectangular paper box and a tall cylindrical can. These were papered inside and outside as neatly as possible with white paper. This particular size of vessel was selected because of the convenience of feeding. Larger vessels however might have been used. In making experiments, these were set at regular intervals on a board 1x7 inches and 5 feet long.

In the first series of tests a bit of food was put into the rectangular box (later in other forms of the set). The board was then placed on the floor at right angles to a line from the monkey to the middle of the board. The operator stepped back a short distance in this line and awaited the monkey's selection of a form. Then the board was placed on a table where the forms were rearranged and a bit of food again put into the rectangular box, after which the experiment was repeated as before. After each test the boxes were rearranged according to a schedule previously made out to insure that consecutive presentations would leave the food box neither at the same place on the board, nor between the same forms. The monkey, being free in the large room, moved about a great deal, so it rarely happened that the board was set down twice consecutively in the same place.

The assumption was that the keenness of the monkey's appetite would induce him to go at once to the form containing the food, if he could distinguish it, and waste no time looking into empty boxes. It would thus be possible by noting his success or failure to judge his ability to distinguish the different forms.

On the average the forms were set down nine or ten feet from the monkey. When he became over enthusiastic it was impossible to get it down more than five feet from him. Whenever he was as close as five feet I set the board down slowly, backing off and compelling him to follow for some distance.

This gave him opportunity to survey the vessels and make his selection. If set down too near him he would rush to the nearest vessel, making no choice whatever. A typical case would be like this: the monkey ten or fifteen feet away, "flea hunting," continues until he hears the board strike the floor, when he rolls up onto his feet and dog-trots over toward the board. The eyes are blinked toward the board as a whole or moved so as to sweep the entire length of it in the fraction of a second. At a distance of from six to ten feet it becomes apparent which vessel he means to try first. Occasionally, however, when the association has been established fairly well, he may come within two feet of the board in the direction of a wrong vessel, and then, as if noticing the mistake, make a rather sudden curve and go to the correct form. Until the association had been pretty thoroughly established the monkey inspected the other vessels after getting his morsel. But when the matter had been well learned he took the food and trotted away immediately. When he came up directly to the wrong vessel, he looked into it with hand up-lifted to take the food. Not finding it he looked into the vessel next to this one. This looking into the vessel was continued until the food was found. If food were in the vessel standing in position (1) at the left of the board for example, and he should come up to the one in position (2) he might go next to (1) or might pass along the six forms in the order, 2, 3, 4, 5, 6, 5, 4, 3, 2, 1. On the return movement (5) was looked into in the earlier experimenting, but later, the return would omit (5) and occasionally both (5) and (4). The tall cylindrical can appealed so little to the monkeys that they rarely went to it directly, but in case of a wrong start, if the can came in between the vessel first looked into and the vessel containing food, it was examined like the rest.

TABLES OF ASSOCIATION WITH FORMS.

(*Male.*)

FORMS USED.	FOOD IN RECT. BOX.			FOOD IN GLASS.						FOOD IN ELLIP. CAN			FOOD IN TRIANGULAR BOX.			
Triangular Box	6	3	1	1	2						4		3	11	26	25
Glass	1				11	19	26	29	30	16	5	1	5	3	1	1
Bottle					3	10	2			1			1			1
Tall Can							1			2						
Elliptical Can	4			1	4	1	1	1		10	21	29	15	9	1	2
Rectangular Box	19	27	29	28	10					1			6	7	2	1

(*Female.*)

Forms.	Food in Rect. Box.		Food in Glass.			Food in Ellip. Can.			Food in Triangular Box.		
Triangular Box	6	2	2						7	10	25
Glass	8		2	22	28	27	14	3	3	1	
Bottle	2	1		2		1					
Tall Can											
Elliptical Can							7	26	12	11	4
Rectangular Box	14	27	26	6	2	2	9	1	8	8	1

The results of the tests have been divided into groups of thirty each. The numbers in each of the vertical columns in the tables above indicate how often the animal went directly to the form in question in the thirty tests. No account is here taken of the forms looked into after the one to which the monkey went directly. Thus if the food was in the glass he may have first gone to the bottle and then to several other forms before going to the glass. In such cases these tables take account only of the look into the bottle.

The original notes were fuller, however. An attendant recorded the results for me. If the animal went directly to the box containing the food this was noted as a success. If, however, he went first to some other form, a note was made not only of the form approached first but also of what other forms were looked into before finally coming to the one containing the food.

The male was fed consecutively 90 times from the rectangular box; afterwards 180 from the glass; 90 from the elliptical can, and 120 from the triangular box. The female was fed from the rectangular box, glass, elliptical can and triangular box respectively, 60, 90, 90 and 90 times. Testing was always continued until the animal could make from 25 to 30 correct choices out of 30 trials. In some of the size and color tests to be mentioned later, when as many as 300 tests were made with no signs of improvement in the association, the experimenting was discontinued without this standard having been reached.

When the food was placed in the rectangular box which was the first form used, the association was practically perfect for both animals in 60 tests. With the male when the food was changed to the glass the table shows the old habit practically broken in the second 30 of trials and the new habit practically perfect in the fourth 30. The female perfected her association with the third 30. When food was put into the elliptical can the old habit was broken up in the second 30 and the new was perfected in the third. In changing to the triangular box the old habit was broken in the first 30 and new was formed in the

third. With the female the change from glass to elliptical can revived the first association so that the rectangular box was chosen 2, 9 and 1 times respectively in the three thirties. When the food was changed to the triangular box this first association was again strongly revived, the rectangular box being chosen 8, 8 and 1 times. A similar revival occurred with the male when the food was put into the triangular box.

Sometimes the changes or breaking of the previous associations were very sudden. In the table it will be seen that the male made 19 correct choices in the first 30 tests when the food was put into the rectangular box; and, again, 10 correct choices when the food was put into the elliptical can. These would appear less abrupt if the results were arranged in groups of 10 each instead of groups of 30 each. In the first of the cases just mentioned the male made no correct choices in the first 13 tests. In the second case 6 of the correct choices were made after the fifteenth test.

The superiority of human beings in breaking such an association may be seen in the fact that my assistant who helped to change the position of these forms and who put the food into the boxes, in all of the above changes, only once or twice threw the food into a box used in a previous series, while the monkeys returned to their former associations from 21 to 36 times. Their success in establishing associations with these forms was greater than with the sizes and colors tried later.

If one takes into account the total number of forms looked into by the monkeys instead of counting only the number of times that they went directly to the wrong one, as I have done in the preceding table, the errors appear to be a little more than three times as numerous. To illustrate, in the first 30 tests for the male as shown in the preceding table, when food was put into the rectangular box, he made 11 wrong first choices, but he looked 25 times into wrong boxes. When the food was put into the elliptical can, in the first 30 tests, while he made 20 errors by direct choice he looked 61 times into the wrong boxes. This method of counting errors, while suggestive, does not seem to me to be the best method of evaluating the reactions. If the food is in the glass and the monkey comes from a distance and goes directly to the bottle, he clearly makes a mistake. He has selected the wrong form and should be credited with one error. If the rectangular box stands next and he peeps into it this ought not to be counted an error since it is not a choice. The box stands next and he goes to it for no other reason than that it is next. This difference between a *choice* and a *mere look* was nicely illustrated with the tall cylindrical can. While it was rarely ever chosen, as the preceding tables show, it was nearly always looked into if a

mistake was made and it came in the line of search. In all the tests following, in sizes, colors and numbers no accounts will be taken in the tables of this extra looking into vessels.

With the male when the food was put into the glass a confusion arose between it and the bottle. This is shown in the table by the 3, 10 and 2 times that the bottle was chosen. The fact that the two forms were somewhat alike will account sufficiently for the confusion.

The monkey's problem in these tests was at first to associate the food with a special form. When the food was afterwards put into other forms the problem became more complex, and the monkey passed through three distinct stages in dealing with it: (1) a series of artless returns to the forms previously used, (2) a stage of confusion in which there was a conflict between the old association and a dim realization that the food was not to be found where it had been before. In this stage the monkey was about as liable to go to one of the six forms as to another. The duration of this period was not always long enough to show in a table made out by thirties. (3) A stage of progress toward perfection in the new association. The processes at this stage seem to follow the same order as where there were no preceding associations to be broken up.

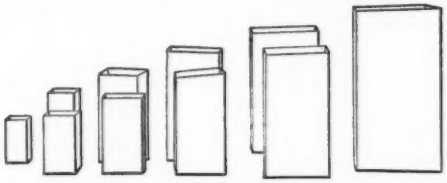
That the animal's choice was not based upon the sense of smell but upon the visual recognition of the form was very apparent. With one exception, when the monkey was searching for food, the head was turned so as to bring the eye over the top of the vessel. In the course of several thousand tests of this general type the monkey only once seemed to try to locate the food by the sense of smell. All discussion of this question, however, is reserved for a section on "The Sense of Smell."

Summary. Restating briefly I may say that the monkeys are able to discriminate these forms and to associate food with them consecutively. The associations are not formed by a single trial, but come about more or less gradually through much repetition. It is easier to form an association *de novo* than to break an established one and form a new one. The necessity of forming a new association induces a revival of former associations of the same general kind. The learning process, upon the whole, is still that of trial, happy accident, recollection of the fortunate movements, and an elimination of the useless ones.

Size Tests.

The tests for discrimination of sizes were made on the same general plan of those for forms. Instead of the boxes there used I made six rectangular paper boxes having an altitude

twice one side of the base. Their heights were 2, 3, 4, 5, 6, and 7 inches respectively. In the cut, which was made from a photograph, these boxes are shown as the smallest and the largest



together with the four others in the rear. The first box was too small to be used for feeding and the last was rather tall. For this reason the monkeys were fed only from the four intermediate boxes. We shall speak of these size-boxes by the numbers 1 to 6 beginning with the smallest.

Although many more tests were made with the boxes of different *sizes* than with the *forms* the association never became so definite. The *forms* used in the preceding series must have been much more readily distinguished than are the *sizes* used here, for while the male made the four associations of *form* with 480 tests, 1,080 tests with the *sizes* sufficed only to induce a general grasp of about such and such sizes. The female was tested with only a part of these sizes, but with these she showed greater ability than the male though she made about the same confusions as he did.

In the form tests they had to associate their food with a particular geometrical figure and had only to look for that one particular thing. No other form needed any special attention. In trying to select the correct size, however, if the monkey was to succeed in choosing rightly, he must compare before he could be more than measurably sure of choosing correctly. Probably the only alternative to such a method was for him gradually to fix upon an absolute size and thus resolve his problem back toward that met in the form tests, though this would be by no means easy. It may be that this is what the female did and thus secured better results than the male did.

The blunders of the monkeys in these size tests are quite interesting in themselves. When the food was put into box 3, 3 and 4 were frequently confused; while 3 and 2 were confused much less frequently, even less often than 3 and 5. The same thing appears throughout the tests. When the food was in 5, 6 was chosen more frequently than 4, even when 4 was much nearer the size of the previous feeding form. This can sometimes be accounted for by the order of the tests. For example, when the food was changed to 2, after being in 3 and 5, on account of

the previous association with these larger forms, 5 and 6 were chosen oftener than 1. But the tendency cannot always be explained in this way. When the food afterward went over to 4, 5 was chosen more frequently than 3; and 6 more frequently than 2. Why should the confusion be always with the larger forms? My first thought was of the psycho-physic law. While the altitudes of the boxes were 2, 3, 4, 5, 6 and 7 inches, the lateral surfaces presented to the monkeys as they approached were, 2, 4½, 8, 12½, 18, and 24½. Thus, while box 1 presented a surface less than ½ of box 2, box 3 was nearly ⅔ of 4; and 5, nearly ¾ of 6. The proportional difference constantly grows less and less. If volumes are considered, the variation is still greater. Then the ratio of box 2 to box 1 is much greater than that of box 4 to box 3, or the ratio of difference decreases as the larger boxes are compared, so that the stimuli for distinguishing boxes become less and less. This confusion of the larger forms would then be in harmony with the psycho-physic law of stimulus and sensation. The less the proportional difference the greater the confusion of forms. As a control test, I constructed a series of boxes varying in volume by the set ratio twenty-one tenths beginning with the first box as before, 1 x 1 x 2 inches. The other boxes like this one were made twice as high as wide. The sixth box was so nearly the same size as the sixth in the former tests that the old 6 was used again. The volumes of these boxes were:

Old series:	2	6.75	16.	31.25	54.	87.75
New series:	2	4.2	8.82	18.52	39.2	82.31

These forms are represented in the front row in the cut along with the largest and smallest boxes as before. It was assumed that if the monkeys selected according to the psycho-physic law in this matter they would confuse the feeding box each time equally with larger and smaller boxes. Here again, however, the confusion was with the larger forms. It may be that the tendency was to choose the larger forms simply because they were more conspicuous. Possibly, in monkey logic, it may appear that the larger forms should bear the most fruit; so that by choosing a larger form more food would be obtained, and that desiring to run no risk of missing food they went to the larger boxes.

Summary. These tests reveal what seems to be an ability either to compare sizes roughly or to fix upon an absolute standard of size. If one recalls the experience with the designs it will not seem probable that this association was based on any accidental markings or other minor peculiarities of the boxes. Their choice does not seem to have been made in any perceptible degree according to the psycho-physic law. Besides these this

study reveals nothing not already shown in the experiments with the *form tests*.

Discriminations of Color and Shade.

The presence everywhere in the animal kingdom of colors, ranging from the most modest to the most brilliant, protective coloration in animals, and the display of colors in animal courtship, would suggest, at least, the possibility that animals not only perceive colors but have preferences among them.

But, except for insects, very little has yet been done to give experimental proof of color perception, and almost nothing to show that the apparent cases of color perception and preference are not based upon discrimination of light and shade rather than genuine color perception. If, for example, an animal shows a preference for a light yellow over a dark blue, one must make sure that he is not merely choosing what appears to him to be a light in preference to a dark gray, instead of one color in preference to another.

Lubbock's experiments upon insects are not complete in that he failed to take this factor into account. In his experiments with ants, for example, he put strips of colored glass over their nests, and after a given time counted the ants congregated under each of the strips. *Formica fusca* congregated under these strips in the following numbers: red, 890; green, 544; yellow, 495; violet, 5. After numerous experiments with bees he says: "It seems to me that the preceding experiments show conclusively that bees prefer one color to another, and that blue is distinctly that favorite." Wasps also, "are capable of distinguishing color, though they do not seem so much guided by it as bees." That these insects have color preferences is probably true, but the experiment would be more conclusive if Lubbock had excluded the possibility of the results having depended upon differences in brightness. The choice of red over yellow and violet may have been due to a difference in the brightness rather than in the color of the glass strips.

The probability is very strong that birds recognize colors and have preferences among them; but, so far as I know, definite experiments are almost entirely wanting. About color perception in the higher classes of mammals there has been a great deal of surmising and guessing. It is said, for example, that horses delight in bright colored ornaments on their harness. But there is no definite proof of it. Mr. Cornish well summarizes current opinion on this matter. He says: "Domestic animals, which see bright colors other than green in large masses more frequently than wild ones, might be supposed to exhibit the consciousness of such differences in the most pronounced way. Yet it is next to impossible to cite an

instance in which a dog exhibits curiosity as to color, or identifies an object by its hue. The writer has seen a setter refuse to retrieve a black rabbit because it apparently thought its master had shot a black cat. But a house-living dog shows no preference for a red carpet or rug over a blue or variegated one, and expresses no surprise or curiosity whether its master wears a red uniform or a black evening suit. Domestic cattle are so much affected by violent contrast of white and dark that the presence of a black, white or very clearly spotted animal in the herd sometimes results in calves being thrown of the same colors or markings. But though red is said to irritate a bull, and to excite hunters by association of ideas, the latter statement rests partly on surmise. They are equally excited by the sound or sight of hounds, or of a number of riders, whatever the color of their coats. None of the cats, whether wild or tame, show any partiality for bright hues; and among all the strategems used from time immemorial by hunters, the use of color as a lure for quadrupeds is notably absent. Many birds, on the other hand, have a marked preference for bright colors, and exhibit strong curiosity when unusual tints are shown to them. Among the less known examples is that of the red-legged partridge. These birds abound in the lower spurs of the Lesghian Mountains, near the Caspian, and the native hunters use a device for killing them based on this æsthetic preference of the partridges. By the door of nearly every house stands a wooden frame, on which canvas is stretched, covered with daubs of brilliant colors. This the shooter carries with him, and sets up in front of him as soon as he has discovered a covey. As soon as their attention is attracted he waits behind the screen, until the whole covey run up to within shot, and then fires through a loop-hole in the center of the screen. The Russian government has now forbidden the use of these colored lures, as the birds were being exterminated. It is probable that the idea of their use was first suggested by the interest the birds took in the carpet frames set up outside the houses for weaving the brightly-colored Shusak rugs."[1]

I am aware of but two series of experiments on mammals, except the *genus homo*, to determine their ability to discriminate colors. One of these was made by Mr. Elmer Gates. He experimented with dogs, using small pans of various colors and shades, and colored metal plates. Food was put regularly under pans of a given color or shade, and these were distributed among a great many other pans of different colors and shades. The metal plates were so arranged in a hall-way, through which the dogs passed, that they had to step on them. Plates

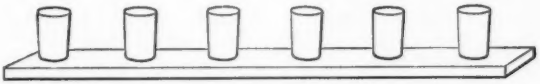
[1] Animals at Work and at Play, C. J. Cornish, p. 130.

of a given color were connected with induction coils, so that whenever the animals stepped upon them a slight shock was received. Mr. Gates believes he has shown in these experiments that dogs not only perceive colors but they also perceive very small differences of shades in colors.[1] The other experiments were made by Mr. Garner. He says: "In order to ascertain whether monkeys had any choice of color or not, I selected some bright-colored marbles, candies, balls, bits of ribbons, etc, I took a piece of pasteboard, and on it placed a few bright-colored bits of candy, which I offered to a monkey and watched to see whether he would select a certain color or not. In this experiment I generally used two colors at a time, and changed their places from time to time in order to determine whether he selected the color by design or accident. After having determined which of two colors he preferred, I substituted a third color for the one which he cared least for, and continued thus until I exhausted the list of bright colors. By changing the arrangement of the objects a great number of times it could be ascertained with comparative certainty whether the color was his preference or not. I find that all monkeys do not select the same color at different times. But I think that bright green is a favorite color with the Capuchins, and their second choice is white. In a few cases white seemed to be their preference. I have sometimes used paper wads of various colors, or bits of candy of the same flavor rolled in various-colored papers. They seemed to choose the same colors in selecting their toys. I have sometimes used artificial flowers, and find that as a rule they will select a flower having many green leaves about it. It may be that they associate this color with some green food which they are fond of, and consequently that they are influenced by this in selecting other things. I kept a cup for a monkey to drink milk from, on the sides of which were some brilliant flowers and green leaves, and she would frequently quit drinking the milk to play with the flowers on the cup, and seemed never able to understand why she could not get hold of them. In one test I had a board about two feet long, and laid a few pieces of pink and white candies in four places on it. The monkey took the white from each pile before touching the pink; except in one instance it took the pink piece from one pile. I repeated this test many times. In another test I took a white paper ball in one hand and a pink one in the other, and held out my hand to the monkey, who selected the white one nearly every time, although I changed hands with the balls from time to time. These experiments were mostly confined to the *Cebus*

[1] Monist, July, 1895.

monkeys, but a few of them were made with *Macaques*. They seem to be attracted generally by all brilliant colors, but when reduced to a choice between two, such seem to be their tastes."

The question has seemed to me of sufficient interest to justify a more thorough investigation, and I have made a somewhat extended series of experiments (6,700 tests) under as favorable conditions as I could command. The general form of the experiments resembled those on form and size. I have used five pieces of apparatus, four of which are described below.



(1) A board 1x7 inches and five feet long. At regular distances six holes were sunk in the board, each large enough to admit freely the bottom of a cylindrical glass. This board held the glasses firmly and yet allowed them to be taken up easily and interchanged. The convex surface of each glass was covered with paper. A bright blue, a bright yellow, a dark red, a light green, a dark gray and a light gray. The brightnesses of these colors were determined by Rood's "Flicker method," with the result that the red was found to be as bright as a gray produced by mixing 35° of white with 325° of black, the blue as bright as a gray with 50° of white and 310° of black, the green as bright as a gray with 117° of white and 243° of black, and the yellow as bright as a gray with 215° of white and 145° of black. The dark gray corresponded to 90° of white and 270° of black; the light gray, to 167° of white and 193° of black.

(2) The board and glasses were the same as before, except that the two glasses covered with gray paper were not used. Each of the above color glasses was used singly with three other glasses covered with gray paper having the same brightness as the color with which they were used, determinations being made as before by the "Flicker method."

(3) The board was the same as in (1) but the six glasses were covered with grays of brightnesses indicated by the following degrees of white as determined with the rotating discs 215, 167, 135, 90, 35 and 0. The sixth above was intended to be black, though it was not absolutely so. It was not measured, but was estimated at not more than two or three degrees of white; for convenience it will be spoken of as if absolutely black.

(4) Nine glasses covered with gray papers having brightness

corresponding to the following degrees of white: 215, 185, 167, 150, 135, 117, 90, 35 and 0.

The method employed in the first series of tests was the same as that used with the forms and sizes. The glasses covered with gray were not used as feeding vessels. The four colors were used in the order blue, yellow, red, green. The main results appear in the accompanying tables which are arranged on the same plan as those under the form tests. The tests have been divided into groups of 30 each. The figures in each vertical column indicate how often the animal went directly to each one of the colors and grays within a group of 30 tests. Again, as explained above, our tables take account only of the glasses to which the monkeys came directly.

It will be seen that the male required many more presentations than the female in order to reach our arbitrary standard of from 24 to 30 right choices out of 30 trials. It will be recalled that the female surpassed the male also in the form and size tests. In the tables for the female we note again the revival of the first association when the third and fourth were being formed. The second association also was strongly revived with the fourth. These revivals were less marked with the male. From these tests alone we cannot infer that the monkeys have a clear perception of the colors, for a discrimination of the differences of brightnesses alone might possibly bring about the results found. The likelihood of the latter alternative is perhaps somewhat strengthened by two relations which appear in the above tables. (1) The association with the yellow was revived more strongly with the green than the association with the blue was. (2) Upon the whole, the dark gray was chosen more frequently with the red and blue than with the green and yellow. It will be seen in the description of the apparatus that the dark gray had a degree of brightness nearer to that of the red and blue than to that of the green and yellow. While the former of these may have been offset by the fact that the association with the yellow was more recent than that with the blue, and the latter may have been made less significant by the fact that the light gray was chosen somewhat more frequently with the darker than with the lighter colors, still there remains the possibility that the confusions named above may have arisen out of a partial or complete failure to perceive the colors as colors.

In order to determine whether brightness or color was the basis of discrimination, four control tests were made. In three of these I attempted to determine whether the monkeys could discriminate grays and colors varying by the same degree of brightness equally well. If blue and red, for example, with a difference in brightness of 15 degrees were differentiated per-

TABLE BASED ON THE TESTS IN COLOR PERCEPTION.

Male.

Colors used	Food in Blue						Food in Yellow									Food in Red												
Red	4	4	3		1	2	3	6	7	1	2	3	2	2	1		6	3	5	7	5	15	12	10	13	14	16	21	27	28
Blue	9	9	12	20	28	24	25	20	13	14	9	6	4	4	3	5	12	7	5	9	7	2	6	3	4	3	8	5	3	2
Dark Gray	12	10	11	7		2		1	3	3	5	6	3			1	3		4	3	3	1	5	3	4	2	2	1		
Light Gray	4	2	3	3	1	2	1	1	3	2	1	2				2	3	2	2	3	6	2		2	2	2		1		
Green		5	1				1	2	1	2	1	3			1	1	2	3	1	2	3	3	3	2		3				
Yellow	1								3	8	12	10	21	24	25	21	4	15	13	6	6	7	4	10	7	6	4	2		

Male. ‖ *Female.*

Colors used	Food in Green												Food in Blue				Food in Yellow				Food in Red			Food in Green					
Red	16	12	12	14	11	7	8	8	4	4	3	2	4	2	1		2	4			6	13	28	25	18	6	3	4	1
Blue	1	2	3	2	2	2	2	1	1		1		8	18	19	26	20	12	5		2	1		1	2	4	1	3	
Dark Gray	4	1			2	1	1						5	5	3	2	1	1			1	2		1			1		1
Light Gray	1	3	1	2	1								5	4	4	2	3	1	1						1				1
Green	5	10	11	7	11	19	18	21	24	25	26	28	4	1	3		1	1	1		1				3	12	19	17	25
Yellow	3	2	3	5	3	1	1		1	1			4				3	11	23	30	20	14	2	3	6	8	6	6	2

The figures in each vertical column indicate the number of times that the animal went directly to each of the glasses within a set of thirty tests.

fectly and two grays differing by 15 degrees, vary imperfectly, then color very probably was the basis of the discrimination in our first series of tests. But if the colors and grays were differentiated about equally well then the question would still be open.

In the first of these control tests apparatus (4) was used. The glasses, numbered from black to lightest gray, 1, 2, 3, 4, 5, 6, 7, 8 and 9, will be referred to by these numbers. These tests were made with the male only. Food was put first into the lightest gray, number 9. It was set down then along with the black, number 1, and the monkey was allowed to take the food. These two glasses were presented together ten times, being exchanged at my back and then set down sometimes by crossing them over and sometimes not. Care was taken to avoid routine and regular position. As far as possible the glasses were put in good diffused light, and in such positions that shadows might not fall upon them in a way to make the lighter seem to be the darker. After feeding in the light gray, in comparison with the black, the black was replaced by the very dark gray, number 2, and 10 more tests were made. In this way we continued down the scale of grays from black to light and back again. To avoid the criticism that the food glass, number 9, may have become soiled or have had some distinguishing mark on it besides the shade, I replaced this glass occasionally and also renewed the paper covering. I could note no difference, however, in the number of correct choices when these changes were made.

Assuming that the first half of these tests (800, or 100 to the pair of glasses) were disturbed somewhat by the mere difficulty of learning the trick, and therefore neglecting these and calculating the per cents of right choices for the last half only we get the following results: with (9 and 1), (9 and 2), (9 and 3), etc., respectively, 100, 99, 98, 98, 98, 95, 87 and 77 per cents of correct choices.[1] When the white (mixed with black to form the grays) differs in two grays by only 48° and 30°, as when 7 and 8 were compared with 9, we see a marked reduction in the right choices. Yet these grays are so very different that the human eye distinguishes them with perfect ease and certainty. The difference of brightness in red and blue, however, was only 15°, but the monkeys distinguished them easily even though they were mixed up with four other colors; whereas here a choice between two grays differing by 30° of white was made with considerable difficulty. If brightness were the basis of the association in the first series of color tests, the red and

[1] The corresponding per cents for the whole 1,600 tests were 97.5, 96, 94, 92, 91.5, 86, 77.5, and 70.5.

blue, differing only by 15 degrees of brightness, should have been confused freely. But they were not so confused. The female, especially, confused these colors but very rarely. We infer, therefore, that the monkeys were probably making their associations on the basis of color and not on that of shade.

This test was followed by a second series of control tests on the following plan, the apparatus being the same as in the preceding tests. I first used glasses 8 and 9 together, putting food in the lighter of the two, in number 9. After 10 tests, 9 was dropped out and 8 and 7 were used, the food being put into number 8, the lighter one. This was continued up the scale of shades and down again, introducing such variations in the order of presentation as would equalize the learning and practice effects.

Taking the results, along with the degrees of white in each of the grays, for the last half of the tests again, it appears plain that in a gross way the per cent. of correct choices agrees with the differences of degrees of white used in forming the grays, and that differences of 27, 32, 35 and 55 degrees only, give results above mere chance; for chance alone should have divided the selections half and half. The difference in the degrees of white between 1 and 2, 2 and 3, etc., respectively, were 35, 55, 27, 18, 15, 17, 18 and 30. The corresponding per cents of correct choices were 94, 83, 68, 52, 46, 56, 50 and 64.[1] These results agree quite well with those of the preceding test, especially if we consider that in the preceding the feeding glass remained of the same shade throughout the whole series of tests. Its image, therefore, may have become somewhat definitely impressed on the monkey. In that event his choices would be based upon an absolute standard in the same way as was suggested in the size tests. In this last series of tests the feeding glass changed with each group of 10 tests. Hence no single image could be of any particular service. It would appear then that if the monkey managed to choose with measurable correctness he very likely had a general notion of a low order, which might be represented by *food-always-in-the-lighter*. The inducement to generalize would be greater in the tests where the change was from the darker toward the lighter grays. For here when 1 and 2 were used the image of 2 might become partly fixed as the feeding glass. But when 2 and 3 were used next the monkey must leave the glass in which he had just been fed and move on from 2 to 3, from a darker to a lighter. That is, shade 2 had to be abandoned and shade 3 selected even though shade 2 was before him. That this was a more severe

[1] The corresponding per cents for the whole 1,600 tests were 86.5, 88, 72, 56, 48, 45; 53.5, and 69.

test than the reverse movement, appears from the fact that here he chose correctly on the average only 69 per cent. of the times, while in the reverse movement, where the lighter glass just used as a feeding glass was each time dropped out and a darker one put in, and where he could, so to speak, lag behind with the lighter glass, he chose correctly on the average 73 per cent. of the times. I do not say that the monkey had generalized, but that we have here something that looks very much like a generalization of a low order. I shall return to the matter in the section on General Notions. The differences of 15, 17 and 18 degrees in brightness with these grays has resulted in utter confusion, while a difference of 15 degrees of brightness between the red and blue resulted in all but a perfect differentiation; hence the inference as in the preceding series is unavoidable, that color was really the basis of discrimination in the color tests.

A third control test was made as follows: apparatus (3) was used to see if 6 grays, varying somewhat more in brightness than the colors did in the color tests, would be selected and associated as readily as the colors were. The shades varied from 215 to 0 degrees of white, while in the color tests the variation was from 215 degrees of white (yellow) to 35 degrees (red). The method of experimenting was the same as that used in making the color tests.

Let us designate the grays from lightest to darkest as A, B, C, D, E and F. Each monkey was fed from the same glass C. It was thought unnecessary to change to other feeding glasses. The male was tested 360 times; the female, 180 times. In the 30 included between the 150th and 180th tests, the male made 13 correct choices; but after that his correct choices fell to as low as 5 out of 30 or to the level of mere chance. In the 360 choices he selected A 27.2 per cent. of the times; B, 18.5; C, 22.5; D, 14.4; E, 8.0; and F, 9.4. Clearly he knew the food was among the lighter glasses, but was unable to distinguish well enough to select accurately. The female in her sixth 30 succeeded in choosing correctly 21 times. She chose A 11.6 per cent. of the 180 times; B, 25.5; C, 43.9; D, 9.4; E, 5.6, and F, 5. In these tests the fact that the monkeys chose B, a glass lighter than the feeding glass, more frequently than D, a darker one, may have been due to a tendency to choose brighter colors and lighter shades, as suggested by Mr. Garner. The male may have been influenced somewhat by his preceding experience in the two series of control tests described above. The female, however, had been fed from gray glasses but 22 times in all, and even then with the food in the darker glass. (See Experiments with Designs.) The results show better association on the part of the female than on the part of the male, but in

neither case does the success compare at all with that attained by each when dealing with the colors. The conclusion that these animals have a clear perception of color and are not dependent upon lights and shades alone, again seems justified.

As a final and crucial test apparatus (2) was used. Shades of gray paper were selected by the "Flicker Method," differing as little as possible from the brightness of the colors used. Glasses were covered with these as before, and a color glass with its three equal gray glasses were used together. They were exchanged on the board and presented in the same way that the colors were in the original color tests. If now the animals do not perceive color they should have been brought into complete confusion with regard to the colors and the equal grays since these necessarily differed very little or none at all in brightness. The three gray glasses were used to give sufficient shifting on the board, and thus to exclude the possibility of choice by mere position.

Sixty tests were made with each color in the order, blue, yellow, red and green, all on the same day. These results are shown in the first of the accompanying tables. The data for the second table were obtained eight days later.

Colors Used.	Male.		Female.	
Blue	17	27	13	26
Yellow	12	23	27	30
Red	26	28	24	30
Green	28	29	30	30

Colors Used.	Male.		Female.	
Yellow	21	27	28	30
Green	27	27	27	28
Blue	26	26	25	30
Red	24	25	26	27

The figures in the tables indicate the number of times that each color was correctly chosen in a set of 30 tests.

In this second series of tests the colors were presented in the order designated in the second of the tables above. Here 30 tests were made with the yellow; then 30 each with the green, blue and red. A second round of 30 each was made immediately afterwards.

The association, with this apparatus, was much more readily formed than in the first color tests when the six glasses were used, partly because the board was now shorter, and partly because there was less of diversity, the three glasses being all of the same gray and there being but one colored glass. The accuracy of the discriminations was so great and the associations so readily formed that there is in my opinion absolutely no question any longer about the monkeys' ability to perceive color.

Color Preferences.

I attempted to study color preferences by feeding the monkeys colored candies, as Mr. Garner did. The candies were handed to the monkeys either on a cardboard or on the palm of my hand. They were separated some distance, and the relative position of the colors was frequently changed. The card or hand was always withdrawn as soon as one of the candies was taken. The male was so much frightened by my coming near the cage that, although he sometimes took the candies, he could hardly be said to have chosen between them. After feeding the female about 100 times she resorted to grabbing with both hands, apparently to make sure that neither of the pieces of the candy should escape her. After the female adopted this method no further tests of this kind could be made with her. No conclusions can be drawn from these tests of mine with reference to color preference. This method of testing in my judgment is very unsatisfactory. A little variation of the card or the hand from right to left may determine the selection, since the distance that the animal moves in getting to them is so small. Further, with a timid animal one gets nothing deliberate enough to partake of the nature of a choice. Most of the animal's attention is given to the experimenter, and it simply makes a quick grab for that piece of candy which is in easiest reach from its position.

Another attempt was made, using a method similar to that employed in making the tests on color perception. I covered the cylindrical surfaces of glasses with the following colors of paper: green, blue, orange, dark gray, red, yellow, violet and white. As to their brightness I will here say only that the blue, red, green and yellow were the same as used in the preceding tests. The orange was slightly darker than the yellow, and the violet was darker than the blue.

Experiments were made with the male first. I presented the four glasses, blue, green, orange and dark gray together. A morsel of food was put into each glass, and the animal was allowed to take all the food before the glasses were taken up. The order of the glasses was changed as before. After 30 presenta-

tions the other four glasses were used. In the first group the first choices were: green 12, blue 1, orange 11, and dark gray 6; in the second: red 2, yellow 12, violet 5, and white 6. Two new groups were now made, each consisting of the two colors most frequently chosen in one group and of the two least frequently chosen in the other group. Presenting the glasses as above, the choices for the first group were: green 5, orange 10, violet 5, and white 10; for the second group dark gray 4, blue 4, yellow 13, and red 9. As a total result so far we have yellow 25, orange 21, green 17, red 16, white 16, dark gray 10, violet 10, and blue 5. Again I threw the glasses into two groups, those most frequently chosen and those least frequently chosen. Now the choices for the first group were: yellow 11, orange 9, green 9, red 1; for second: dark gray 6, violet 7, white 10, and blue 7. The second group here, however, has no value as after about 8 choices he fell to starting in with the glass on the right and to taking all as they came.

The female from the very first began at the left of the row and took the food from each glass in the order of its position. If she has any color preference whatever the association of order completely covered it up. Still hoping to get some signs of color preference from her by this method I selected the two colors which had appealed to the male most and least—yellow and blue. In 30 presentations of these, the food in both glasses, and alternating them right and left, she chose the left glass 28 times. Evidently this indicates a continuation of the former habit. In the other two cases a little noise made by the male in his cage veered her over to the other glass. In experiments of this type, color preference would probably be based on either an æsthetic taste or on the fact that certain colors lie nearer the animal's feeding instincts than others do.

One more series of tests was undertaken in the study of color preference. A cage three feet long, ten inches wide and fourteen inches high was so constructed that the back part could be moved as a sliding door. The inside was painted a light gray. This gave the inside of the box a uniform appearance with no attractive points for gnawing or escape. Eight inches in front were hung two large sheets of colored gelatine—blue and orange. The arrangement was such that very little light could enter the cage other than through these sheets. A vertical partition opposite the middle of the front of the cage and between it and the sheets of gelatine, greatly reduced the animal's opportunity, when in one end of the cage and behind one of the colored sheets, to see and be influenced by the other color. The cage was so placed that direct sunlight flooded the whole of it. One animal was turned into the cage at a time and kept there for twenty minutes. After ten minutes the places of the col-

ored sheets were exchanged. For observation I sat some distance in front of the middle of the cage. By means of the cumulative stop-watch the total time that the monkey spent behind each color could easily be kept. The assumption was that if either color was more pleasing, or less painful to the monkey than the other he would make choice between them and sit in the color most agreeable. The female was in the box during 14 ten-minute periods. In 5 of these she spent more time behind the blue than behind the orange, and 56 per cent. of her total time was spent behind the orange. The male, likewise, was in the cage 14 times, 9 of which were taken up mostly behind the blue. He spent 62 per cent. of his whole time behind the blue. The results were regarded as so very irregular as to indicate neither preference nor the absence of it, and further experimenting by this method was abandoned.

The first time the monkeys were turned into this cage they seemed to be surprised at the change of color on their own bodies; for several times they looked strangely up and down and along their arms, and tried to pick the color off of their hands. This apparatus, in addition to appealing to an animal's æsthetic taste and instinctive habits of feeding, might appeal, also, to his instinctive fear. A timid animal might stay where he considered himself best concealed, and a bold one where he could get the most satisfactory view of his enemies and environment.

The results of these tests on color perception and color preference may be summed up as follows:

1. There can be no doubt that monkeys perceive colors.
2. Two grays having a given degree of difference in brightness are not discriminated as well as two colors having an equal difference in brightness.
3. For accurate discrimination of difference in brightness a difference of about 35 degrees or 9 per cent. of the white constituent of the gray is necessary.
4. The monkeys are able to distinguish colors from grays though the brightnesses are the same.
5. The male appears to have a preference for bright colors, but blue seems to be discriminated against.
6. In two instances there were indications of at least a low form of general notion.

A CONTRIBUTION TO INDIVIDUAL PSYCHOLOGY.

By NORMAN TRIPLETT, Ph. D., Kansas State Normal.

(By way of introduction the following letter may be reprinted here.)

EMPORIA, Kan., March 1, 1902.

Dear President Hall:

At the close of a recitation in Psychology a few weeks ago, in which the subject of Number Forms was discussed, Mr. Arthur M. Clark, a member of the class, came to me and said that he had a peculiarity of a greatly different sort. He spoke of this as if half ashamed of it, and said that while he had had it all his life, he had kept it a secret and had never before even hinted of its existence to any one. Inquiry disclosed such a remarkable system of dealing with letters that I have made an effort to unravel the matter, and I send you herewith some account of it. It is told in the first person though the facts given have in large part been obtained as the result of much patient questioning. Extensive as seems the matter presented I can verify Mr. Clark's statement that merely the simple skeleton is here given. He says that the ramifications and their uses extend it unendingly, and that it would fill a book. The first impression one gets from it is that it is an incoherent jumble of nonsense, and indeed it is, so far as any practical use is concerned. More careful study, however, shows it to be a system that, for the most part, is logical and consistent with itself, and that any one might learn, at least in its simplest form.

Three aspects are to be noted in the account given. 1. The origin and development of the various forms or contests. It is a life history. In the increasing complexity of the system may be seen the influence of environment on the growing child. His interest in the letters was the wax on which everything left its impression. 2. His attitude toward the subject from the time when he first became conscious of his peculiarity. He has always regarded the matter with a good deal of mystery. He insists, and is impressed with the fact, that the forms of the contests described grew up without conscious attention or formulation, and in operation went through their changes as mechanically as the changes produced by the moving picture machine. His facility in running a word through the succession of stages necessary to his final result is wonderful, though lately it must be done by a conscious effort, he tells me. When I commented on his power in this regard he said he ought to be able to do it when he had seen it going on all his life. The word picture which he carries of many words is a letter group which means nothing to the uninitiated. Call a word and he instantly responds with the combination making up his word form. Formerly there were large numbers of these in his vocabulary, as most words when first learned passed through the changes and kept the form. In some cases the word has been arrested at an intermediate stage. At the present time, though many yet remain, the regular word has come in most cases to take the place of the old combination. 3. The personal character given the letters from the association made with the pictures from which they were learned, and their peculiar habit of appearing in the character of hypnagogic images, if such they were.

Mr. Clark is a good student, honest and of strong religious character, and aside from the matter mentioned would not be considered peculiar. He says that as a boy he was delicate and played by himself a great deal.

Yours very sincerely,

NORMAN TRIPLETT.

When I was a boy of perhaps four or five years of age, my older brother came home from school one day and told me of the way his teacher had given for remembering the spelling of the word "Aaron." It was: "big A jumped upon little a and crushed out 'ron.'" This led me to think of the letters "A" and "a." What relation have they to each other? Why is one larger than the other? It was from questions like these that the conception of a struggle among the letters, as described below, arose in my mind. Another element in the process of endowing the letters with force or personality, I believe, came from the alphabet picture books where the letters are pictured in different characters. From these sources the interest in the letters developed which has been so large a part of my life.

The struggle referred to is not a struggle of the letters as they appear in the alphabet but as they appear in words; that is, it is a war of words. The struggle may take any one of four forms according to the word to be dealt with. The four forms, however, developed at different periods in my life and from different causes. In order to understand the struggles of the letters it is necessary to know the values attached to them. The alphabet appears to me to be divided into series of three letters each, preceded by the capitals A B C, as follows: ABC, abc, def, ghi, jkl, mno, etc. In the series "abc," "a" is worth two of "b" and "b" is worth two of "c." "c" in turn is worth two of "d," the first member of the next series, and so on throughout the other series. "a" and "b" taken together are equal to "Cc," "bc" is equal to "ad," "ac" is equal to "bb," "bd" is equal to "cc," and "cd" is equal to "be," and so on through all the series.

Now in this scheme d, g, j, etc., the first members of the succeeding series are regarded as the descendants of "a." The same relation exists between "b" and e, h, k, etc., and also c and f, i, l, etc. Capital A, B, and C, are considered a degree higher than small "abc," and large A holds the highest possible position in the scale of values. These three capitals are not much used, but capital A in particular is necessary when the method employed in treating a word is the one which reduces all letters to the letter "a." Each series of three letters decreases in value as its distance from ABC increases.

In my different systems of treating the letters of words certain terms are used to designate the various processes and these

require definition and explanation. "Combination" is fundamental. It involves the arrangement of the letters to the best advantage for the purpose required. Three principles are embraced in it: 1. the arrangement of the letters for expansion in the manner described later; 2. placing two letters of the same kind together to make one of the next higher, for example, "cc" equals "b" etc., and 3. is combining a descendant of "a" with a descendant of "c" which would be worth, as said before, two descendants of "b." When two letters are combined to make one of the next higher rank, the single letters lose their own characteristics and assume those of the letter that they are combined to make; thus, in "cc" equals "b" the combination "cc" is treated as "b."

"Expansion" is the term employed for the following process: if, for instance, the syllable "ac" occurs as in "accept," it will expand into "acbd," because, according to my system, "ac" always takes "bd" after it when expansion is used; "a" has the same relation to "c" that "b" has to "d," from their place in the alphabet, as before stated, and the product of the means is always equal to the product of the extremes. So in the word "baby," "ba" would be preceded by "dc" and the reason for this change in position is that "c" or its descendant must always precede "b" or its descendant and the product of the extremes again equals the product of the means. Where in any word, the letters are all used in other combinations, leaving "a" standing alone, the "a" assumes to take a "c" or its descendant and expands into "acbd," or it will take the equivalent if corresponding letters of other series are used. For instance, the word "Bella" will be seen in the mind to take the following form, "lblea." The change of position of the letters "bell" is made because "c" or its descendants must always precede "b" as stated above, and the letters "ll" are descendants of "c." The "a" left standing alone will assume to take "l" for some reason not understood, and the "al" will expand into "albm" and the word becomes "lblealbm," and this is my mental image of the word Bella, and in like manner are treated all words in which the principle of expansion is used.

"Suppression" is a process used in connection with "expansion," and is made use of when the system of expansion produces more letters than is necessary. In expansion four letters are always involved; by suppression this number may be reduced to two. For example, in the word "railroad," since "r" from its place in a series is the descendant of "c," "ar" ("a" of course preceding a "c") may be said to be equal to the letters "bq," since "ac" = "bb" and "ar" will equal "bq," "q" holding the same position as "b" in its series.

Then the word "rail" appears as "bqil," which, when the letters assume their proper order becomes "iblq." The explanation being that, as said before, "i" corresponds to a "c" which must always precede "b," and "l" precedes "q" for the same reason. The word is then seen to be equivalent to four descendants of "a," (for "ib" corresponds to "cb" and so in like manner does "lq,") and "cb" = "ad" and "d" is a descendant of "a," and thus the word "rail" is equivalent to "aaaa." "road" the second part of the word, appears as "ardo." Now the "ar" expands into "arbs" ("r" standing for a "c" and "s" being first in its series and hence standing for an "a"), and the "do" expands into "doep," for some letter must go with "o" that will make their product equal to "d" by some fourth letter which is a descendant of "a" below the second term, *i. e.*, the third and fourth terms, respectively, are the first letters below the first and second terms, repectively. This is a general law or principle true for all cases. By the method explained above "bqil," "arbs" and "doep" are each proved to be equal to four descendants of "a," and by combining the results the entire word "railroad" is seen to equal twelve descendants of "a." This has been reached by the method of expansion. Given a game of words in which "railroad" or any word that makes 12 descendants of "a," is found when the game requires but 10, "suppression" may be brought into play to reduce the number. In the word railroad suppression may be used either on the "do" or on the "ar;" that is, the "ar" may be expanded as "arbs," and the "a" and "s" dropped when the whole word becomes shortened to "iblqrbdoep," and we have the ten descendants of "a" required. This may, in like manner be reduced to eight, by applying the method of suppression to "doep" that is, by the cancellation of "d" and "p," "oe" is left and the word becomes "iblqrboe." Which form the word will finally take in my mind, however, depends upon the number of descendants of "a" required at the given time in connection with some other word or phrase, for in some instances more than one word is required in developing a certain method. Some words are rigid and furnish just a definite number of descendants of "a," while others, like the word "railroad" used above, are flexible and accommodate themselves to the needs of the given case.

"Promotion" is raising a letter from one series to the one next above in rank. Skipping a series is not permitted; but promotion must be made by moving through one series or group of letters at a time. "Demotion" is reversing the process. The two processes are inseparably connected, promotion of one letter requiring the demotion of another of the same line of descendants, or of the letter used in connection with it.

The object of the processes named is to bring into the same series the letters being combined. For example by expansion in the manner indicated under the explanation of that term, the word daily takes the form "diejalbmy." Suppose we wished the "diej" and the "albm" to be reduced to the same series: Demote the "a" of the second group to "d," its first descendant; the "m" will be promoted to "j" a series above it in rank; the "l" used with the "b" may be promoted to "i," the first series above it, causing "b" to be demoted to "e," the corresponding letter of the series next below; and the "albm" becomes "diej," and hence the form desired as to series.

In "diej," by demoting the "d" to "g," the "j" may be promoted to "g." The process of changing the "d" and "j" to "gg" are simultaneous or seem to be so. Since "ie" is equal to "dj," it also is immediately thought of as "gg," and as the "albm" likewise became "diej" it also dissolves into "gggg" and my mental picture of "daily" becomes "gggggggggy."

The principles thus explained are used in several ways and these I call games or contests. These games show a process of evolution in my mind, after using one for a few years till the novelty wore off I would take up another. I did not originate these contests, however, not consciously at least. They simply grew. The variations which have appeared seem to have been suggested by different causes.

Contest 1.

A few years after I had begun to speculate about the mysterious character of letters as mentioned above (I presume I was about seven or eight years old), my grandfather told me a story, the thought of which was that no force exists so great but a greater may be found. I think it was a poem, and as I remember it an ant was devoured by a dove which in turn fell a prey to a hawk. The hawk was then captured by an eagle, and the eagle next fell before the arrow of a hunter. Suddenly this game presented itself to my mind as an illustration of the thought of the poem.

It consisted of a contest between two or more words where several words stand for the same thing. As a result of the contest the word that won out seemed to recommend itself for preference among synonyms.

Recalling the division of the alphabet into series of three letters as "abc," etc., in this contest every descendant of "a" or letter which stands in the position of "a" in a series, is thought of as "a," every descendant of "b" is thought of as "b," and every descendant of "c" is thought of as "c." Other letters

are not taken into consideration as such, but are regarded as so many of "a," "b," or "c."

Let us take for illustration "mind" and "brain." Though these words are not synonyms, I formerly thought of them as such. These words became engaged in one of the many contests of that period, to determine which should have the preference in use. Their values were found as follows: the letters in the word "brain" took the order "a-rb-in." Since "r" and "i" are descendants of "c," and "n" the descendant of "b," "r" and "i" preceded "b" and "n," as a "c" must always precede a "b" as stated earlier. Making the proper substitutions for the values of the letters we had "a–cb–cb." Since "cb" = "ad," the word equals "a–ad–ad," but since in this form of contest all descendants of "a" are considered as "a" the word became "aaaaa." If the word reduced to five of "a" or a multiple of it, the combining process was indeterminate. If carried further the process was as follows: "aa" would combine into a "c," so two pairs of "a" from the five were each combined into a "c" giving "acc" and the process could be carried on with the three letters unendingly. At present this is not done, but it was followed till the principle was seen.

The order of the letters in the other contestant (the word "mind") remained unchanged, because the combinations occurring in it were already perfect for this contest. "m" is a descendant of "a," "i" is a descendant of "c," "n" is a descendant of "b" and "d" is a descendant of "a." The word is practically the same, therefore, as my regular group "acbd." Since "cb" = "ad" the word is equal to "ad–ad" or "aaaa." My mental picture of this word is regular, that is, I see the word "mind" because the letters stand in their proper order.

When a word has been reduced to "a"s, the number it contains whether odd or even, must remain so throughout the entire process. Sometimes the word will not reduce to "a"s, in this case the value must be determined by other methods. A word that reduces to four "a"s as was true in the case of "mind," is inferior, because to continue you must reduce both pairs of "a"s to "c"s, so the "aaaa" would become "cc" but this is equal to "b" and an end is reached; while in "brain," as was seen, the combinations may go on indefinitely and it is therefore a better word to use.

The contest just described is an actual one. A teacher once told me to "use my brain" and at another time to "use my mind" in the performance of some mental task, and thinking they were synonyms I wondered which was the better word to use. The words were therefore tested by the method described and as a result I decided to use my brain. Such contests were

always going on in my mind but I remember this case particularly.

CONTEST 2.

This contest is similar to the first. I think it originated when I was about nine or ten years old and had begun the study of United States money. The idea of fives and tens, on which the system is based, appears to have been carried over and merged with my interest in the letters. The fact that five is the number most agreeable to me seems the result of the former connection. I like numbers in which the sum of the digits is five or a multiple of five, as 1900, 122, etc. Numbers below 100 are always thought of as a certain number of fives and a remainder. Other than as stated I have no number form.

The object of this contest is to reach a balance, or to reduce words to five "a"s or a multiple of five. Here, also, the descendants of "a," "b" and "c" are thought of as "a," "b" and "c," just as in the previous contest. By proper combination five "a"s will become "A,".and the value of a word is determined by the number of "A"s and the fractions thereof. "a" is considered worth one-fifth of "A," "b" is worth one-tenth of it, while "c" is worth practically nothing on the last round, since if a "c" is present the "ac" equals "bb" and "bb" equals "a" for two-tenths equals one-fifth. The "c" is therefore practically eliminated.

To illustrate this form of contest take the words "diet" and "food." "Diet" appears as "di-et." "di" expands and becomes "diej" and then the word appears as "diejet." The "diej" is equal to "aaaa," for "d" and "j" are each descendants of "a" and therefore in this system equal to "a." Further, "ie" is equal to "dj," by reason of its position, "i" being a descendant of "c" and "e" a descendant of "b" and therefore equal to "aa." Since "e" and "t" are descendants of "b" they are from their mathematical relations, when combined, equal to "a" and thus the word "diet" equals "aaaaa" which are equal to "A."

Taking the other contestant, "food," the word first assumes the form "doof" the "f" and the two "o"s are each descendants of "c," and "d" is a descendant of "a" so the word next appears as "accc." The "a" taking its proper place before "c." The "a" taking its proper place before "c" is combined with one "c." The "ac" thus combined is equal to "bb" and the word then appears as "cbcb," by the two "c"s taking their usual place before the "b"s. As "cb" equals "ad" the form changes to "adad." Since "d" is a descendant of "a" the word becomes "aaaa," which is less than "A" by one-fifth. "Diet" is therefore the winner in the contest

and is to be chosen in a selection of synonyms. In this case, however, though "diet" was thought to be the proper word, common usage and other considerations prevented my using the word very much. But in general my choice of words is determined in the manner indicated.

Contest 3.

This contest had its origin when I was about the age of ten or twelve, after I had learned the game of checkers. Its object is to promote as large a number as possible of the letters of a word to "A." In order to promote one letter another letter must be demoted as far as the first is promoted. The letter must not be eliminated, however, *i. e.*, promotion and demotion must remain within the limit of the alphabet. One vantage play in checkers is to get as many kings as possible, so, in this game, a letter promoted to "A" is considered a king and the steps taken by a letter to gain the rank of "A" are thought of as moves toward the king's row. The process involves the changing of the letters to "a" descendants, then follows the possible promotions to "A." The word having the most "A"s is the winner.

I was told that all good generals were good checker players, so the first words entering into this form of contest were the names of generals. For instance, the words Grant and Lee, were thus used. Grant takes the form "ag–rn–t." In this combination "rn" equals "ms" because "rn" is the central part of the expansion "mrns" made by expanding "mr" in its usual way, as explained under the term "Expansion" and its equal "ms" is substituted for it. The word "Grant," therefore takes the form "agmst." Demotion and promotion produce the following changes: "s" is demoted to "v," and "a" is promoted to "A"; "m" is demoted to "v" and "g" is promoted to "A"; the final form of the word "Grant," therefore, becomes "AAvvt." The word "Lee" takes the form "le–e." The "le" is the central part of the expansion "dlem," made by expanding "dl," and thus "dm" is equal to "le" and is substituted for it. The word "Lee" now takes the form "dme." The "m" is demoted to "s" and "d" is promoted to "A" and the group becomes "Ase." We thus see that the word "Grant" is the winner, as it equals "AAvvt" while the word "Lee" equals "Ase."

Another element that seems to have entered into this third form of contest was drawn from political life. It was about this time that political matters began to arouse my interest. It was noticed that candidates for office were generally promoted step by step, and that the success of one candidate meant the dis-

placement of some other man. Thus the names of candidates and political leaders also became material for contests.

CONTEST 4.

These contests, as has been said, have been an evolution. This fourth and last is the highest form, and to me the most interesting. It seems to have originated a few years ago, at the time when I first watched a football game. I did not understand the game, but saw that the men were placed to the best advantage, and as this always seemed to be true of the letters, a contest was evolved in my mind. After I had slept a night or two, it worked itself out. I did not consciously do any formulating of the game. For a long time I enjoyed watching the moves and changes of the new contest for the novelty of the old games had from long use worn away.

Football seemed to me a mere trial of strength, though the men appeared to be massed at one point, and when this new contest came up the letters had the same characteristics as the players. The object of the contest was to convert the letters of opposing words into descendants of "a," to bring them as near to "a" as possible and yet remain together.

As the games of football were between educational institutions, I at first found myself working with the names of these institutions; as Yale vs. Harvard, Princeton vs. Dartmouth, University vs. Normal, etc. In the first named, Yale vs. Harvard, the letters of the first word happen to be such that they are permitted to retain their regular order, "y-al-e." The "al" is a combination that expands always into "albm" and the word becomes "Yalbme." The next change that I saw was the combination "Ygggge." Those grasping the principle will see that "lb" is equal to "am," by promoting the "m" to "g" the "a" is demoted to "g" making similar changes with the "1" and "b," and massing, our four group "albm" became "gggg."

In the case of Harvard the letters also retain their natural order and the word appears as "H-ar-v-ar-d." Each "ar" being the nucleus for an expansion becomes in the regular way "arbs" and the word Harvard is transformed into "Harbsvarbsd." Now since "rb" equals "as," by promotion and demotion, each pair becomes equal to "jj;" and by massing we have the aggregation which was seen as the third word change: "Hjjjjvjjjjd." To reduce this to "g"s, the principal element in the combination resulting from the word Yale, we promote each alternate "j" to "g" and demote the remaining "j"s to "m." Placing like letters together we have "Hggggvmmmmd." Harvard is the stronger word, for the result shows as many "g"s as in Yale and of the remaining letters more are of a

higher order. This result is not attained because the original word had more letters than its antagonist Yale but because its letter combinations happen to give more opportunities for expansions. It is this factor, indeed, rather than the length of the word which determines the outcome in a test of strength. Let us take to illustrate this the words Normal containing six letters and University containing ten. Applying the method explained we find that Normal is the stronger word for it contains more letters of a higher order than the latter. The form for Normal being "jjjjjjmmpp," that for University being "mmmmmvvv."

The whole subject in its various forms, as outlined above, may be only a substitute for the day dreams so common among boys. I spent the time with this that, I presume, most boys spend in day dreaming. I remember having only two or three day dreams. It has never bothered me in study hours for as soon as I wish to study it vanishes leaving my mind clear.

In order to develop the results attained with words it was only necessary to direct the attention to the words when the letters seemed to take their different forms much as the images of a vitascope. A word in process of change did not take its ultimate form at one move but the changes were made as explained in the contests, and each step was separate and distinct, though very rapid, as the final result seemed almost instantaneous. After the changes began my mind seemed to be passive and I was merely an onlooker and not a conscious director.

Until I was ten years of age I was content to watch the contests without trying to inquire into their meaning. I then began to question myself as to the meaning of it all. No solution was reached. The idea, however, came to my childish mind to make the following application of it: I resolved to cram myself with facts and to learn all their was to know, thinking that when I was asked a question I could direct my attention to it and the answer would unravel before my mind's eye without effort on my part. In accordance with this plan I conceived the idea of mastering the text-books at school and at the same time of acquiring other subjects at home, beginning with the Bible. I thought that by memorizing everything as I went along, it would be unnecessary to give it further thought as it would present itself of its own accord when needed. The plan was carried out most completely with History. Soon I could repeat the account given in Barnes's History of the Revolutionary War, beginning with "England treated the settlers as an inferior class of people," etc., and continuing to the close of the war. I committed portions of the Bible in the same way. I expected to memorize the whole of it and thus become an authority on the subject. So in other fields of knowledge.

This idea thus became an incentive to study and I worked faithfully from about ten to fourteen when the reaction set in. I saw that the task was too great and became discouraged. I decided that this was not the solution of my problem and cast about for another. By centering my mind upon the problem for some time the means of solution would seem to accumulate and I thought that if I could hold my mind on it long enough that all the material for solution would be gathered up and then must unravel before my mind. At times I would appear to be on the verge of success, but the thought of being so near the goal would divert my mind and the accumulated material would roll up in a ball and burst as a bubble. After an effort of this sort it would be fruitless to attempt it again for a week.

For two or three years past I have endeavored to rid myself of the whole matter, as I gave up any possibility of a solution and thought it useless to have the mind burdened longer. I found it difficult to do so, however, and my mind continued to be occupied with these peculiar contests until I began to write about the subject some weeks ago, since which time it has cost an effort to think of it all. The word formations now require a conscious process, *i. e.*, they will not arrange themselves of their own accord as formerly.

Another phase of my deep interest in the letters and especially the letter "a" remains to be mentioned. From the very first the letters seemed endowed with life, and for many years on retiring at night they would all gather around my bed. Each letter always retained its own personal characteristics. These they seemed to have derived from the letters in the A B C picture books given me. When they first began to appear I could see the letters shining through them; these faded in course of time and only the figures were left. Some of the letters were brownie-like figures, others were animals. "a"s body was the shape of small "a," "H" was a hog, "K" the "kicker"—always ran around with his right leg held stiffly in front, etc. After assembling they would all perform the most amusing acts which would be remembered and laughed at for days. I never mentioned the matter to any one else, however, because I dreaded that people would think I was silly.

It was long before I discovered that I was "a" or rather that "a" was impersonating me. I noticed that while the other letters were going through their own particular performances "a" often took the part of an observer. He was a special character and his many funny actions were differently regarded by me. I could see that he went through at night what I had done during the day. If I had any mishap "a" would incur the same misfortune at night. Some of these are still vivid in memory, many are forgotten. I remember riding

home on a load of hay over a rough road, and when I had retired I saw "a" perched upon a big, jolting load of hay hanging on to the center pole for dear life. One day I lost my temper with a schoolmate. The teacher gave me a severe lecture and told me that if I did not control myself my head would grow out of shape. When the figures appeared that night I noticed that "a" was in great trouble and was not joining in the good time the others were having. His head had become elongated and he would take his hat off from time to time and groan. Subsequent fits of temper on my part always caused further additions to his long head. On one occasion he thought that if he could tie his hat down tightly to his head he could prevent any further swelling. I became angry at something, however, and his growing head lifted the hat so high that it caused the string under his chin to choke him. He prevailed upon "e" who was a dancing lady, to cut it with her scissors. All this doubtless seems fanciful to others but it was very real to me. It was an ever-changing drama enacted for my own especial benefit and I enjoyed it. The figures would at times become engaged in quarrels and would do and say very funny things. I could hear the talking very distinctly.

LITERATURE.

An Introduction to Psychology, by MARY WHITON CALKINS. The MacMillan Company, New York and London, 1901.

"The book is written in the conviction that psychology should study consciousness, both as a series of complex mental processes, or ideas, and as a relation of conscious selves to each other;" there are, therefore, to be considered both "the psychology of ideas, the study of succeeding facts of consciousness without reference to conscious selves, and the psychology of selves, that is, the study of consciousness as the experience of related selves." The first large division of the work separates the treatment of "structural elements of consciousness" and of "concrete conscious experiences." This two-fold investigation of the "normal consciousness" is followed by a briefer account of "comparative psychology and abnormal psychology." At the end of the book stand an appendix and a bibliography.

After twelve pages on the "nature and methods of psychology," the student (the volume is intended to be a text-book) begins, at once, the study of sensation. A brief page hints at the nature of psychological analysis, without, however, making clear the essential and vital distinction between an analysis of 'objects' and an analysis of mind—as when the author speaks of the sensational elements of "rich color and graceful form" (150). It is, clearly, incorrect to speak of structure of mind as the collection of heterogeneous bits and scraps of the 'outside' world. Such a confusion is especially deplorable in a text-book for elementary students.

Most of Part I is devoted to a discussion of the various sensational elements. The definition and discussion of 'sensation' and 'sensation element' are postponed until the concrete facts are set down. The consequence is that the reader knows only by looking ahead exactly what is the author's distinction between the 'element' and the 'sensation.' Other elements are the 'attributive' elements of pleasantness and unpleasantness, the "feeling of realness" and the various "relational elements." The treatment of the elements is followed by a brief chapter on attention, in "its primary meaning of clearness or vividness,"—"a simple relational experience."

The point of greatest interest in this part of the book (Part I) is the author's development of the elements of consciousness. The outline departs but little from the scheme proposed by Doctor Calkins in her paper on "Elements of Conscious Complexes" in the *Psychological Review* (VII, 377). The element of consciousness is defined as "a distinct and further unanalyzable feeling or fact of consciousness." Sensational elements are distinguished in several ways: (1) they are "present in every concrete, conscious experience" (psychological criterion); (2) they correspond to some assignable change in peripheral end organs (physiological criterion), and (3) almost every sensational element has a distinct physical condition (physical criterion). There are three sub-classes of these elements; qualities, intensities and extensities. "The fundamental ground for this division is the observed distinctness of these groups of elements." But the division rests, also, on the two types of serial arrangement under which the elements fall; direct serial arrangement, in the the case of intensities and extensities, and indirect, in the case of qualities.

Curiously enough, the author's elaborate classification of elements is drawn very largely from James's Principles. The sensations are said (114) to correspond to his "substantive facts of consciousness;" the attributive elements (affections) are opposed to the substantive states (because they seem "'to belong' to other elements"), and the relational elements are made to correspond to the "transitive states" of the Principles (133). It may, however, be remarked, in passing, that it is not at all clear that James conceives of these states as classes of 'elements.' In fact, he speaks of the substantive states ("lingering consciousnesses") as sensations, images, percepts, concepts and thoughts (I, 247). It is to be noticed that he is giving the characteristics of the continuous 'stream of thought,' among other characteristics the "different pace of its parts," and is, apparently, not making an analytic quest for ultimate elements. Miss Calkins, it is true, uses the term 'element' in a new sense, but not in the sense of the 'lingering consciousness.' Any final qualification she conceives as an element. She forfeits her right to the term with its old connotation when she passes (*cf. Psych. Rev.*, VI, 507) from irreducibility—the usual mark of the element—to simple characterization or qualification (*cf.* Hume's discussion of *distinctio rationis*). This modification of meaning disregards, it is clear, the very important difference between analyzability and a plurality of properties or characteristics; between a subject—to state the case in logical terms—that can be expressed in simpler subjects and one that is capable of receiving more than one predicate. It is, *e. g.*, no more a dissection of the sensation 'red' to add that it has intensity and extent than it is a dissection of the element 'oxygen' to add that it has the atomic weight 16 and possesses an affinity for free hydrogen. If one were really dissecting, one would be able, in the first instance, to put intensity and extent together and get 'red,' and in the other, to add atomic weight and affinity and produce oxygen. There is, without doubt, an essential difference, from the point of view of analysis, between the qualitative moment and the other moments in the sensation. The author has, herself, hinted at this essential difference by saying, after James, that one has continually 'more and more' as one rises in the intensive and the extensive series, but 'more and more difference' in the qualitative series. Now this is very much like saying that in one case the 'thing,' the 'element,' is not changed—only its amount is changed; while, in the other, a new element is continually appearing;—an element more and more different in kind, not in amount. The 'extensity' series of elements meets with the special difficulty that any member of such a series is divisible and, therefore, complex. The same argument cannot be made to do duty against 'extent' conceived as an attribute, for subdivision does not, in that case, affect the simplicity of the element. This is quite apart from the question of the doubtful propriety of making extensity (if this is to mean more than the invariable arrangement of qualities in a peculiar series) an analytic determination.

Since the sensation element is a single characteristic of the sensation, it is somewhat surprising to find that a similar analysis of the affections (attributive elements) is not attempted (123). Surely, no one would deny that pleasantness and unpleasantness of are varying intensities. Experiment shows, *e. g.*, that it is perfectly easy to initiate comparison of the degrees of affective qualities. Again, it may be urged that since the affections are 'attributive,' their dependence upon other contents is a separable characteristic which destroys the elemental nature of the affection. The same point may be raised against the author's 'relational' elements. These are characterized not only by their quality (*e. g.*, 'like,' 'more,' 'one'), but, also, by

an unfailing dependency upon more than one other conscious experience. Miss Calkins, herself, speaks of this as another "characteristic;" although she makes it, in the writer's opinion, wrongly, a 'reflective' characteristic. But if reflective then extrinsic, and if extrinsic, a mere external connection.

The *Introduction's* neglect of the temporal aspect of consciousness is decidedly unjust. No mention is made of it, so far as the reviewer remembers; although it is discussed in the earlier article on the attributes (*op. cit.*, 510).

It is entirely natural that the volume under discussion should pay little attention to sensations for their own sakes. They are simply "complexes of invariably combined sensational elements" (109). How the color is 'bound up' with the brightness and bigness; what manner of connection obtains within the sensation, we are not told. To be sure, this is said, later, to be a case of "fusion," but fusion, like "association," is, for the author, an objective, 'reflective,' not an inherent form of connection. The distinguishing characteristic of the fusion—"the synthesis of peripherally excited, conscious elements"—is said to be physiological: "each one of the combined or fused elements must be directly excited by the stimulation of an end-organ." There is really a rare opportunity for some one to write a systematic account of the simpler conscious complexes and the various forms of connection. The *Introduction* disposes of the problem by saying that synthesis may be regarded either subjectively or objectively. If it is approached subjectively, introspectively, all that is found is a "relational feeling," a feeling of connection which is prominent "in judgments and general notions," but "is 'swamped' in sensational and affective elements," in the case of percepts, emotions and images. Looked at objectively, "the connection between processes is not a peculiarly psychic phenomenon, but is a general fact, common to every science. . . . . It is not immediately realized, but is reflectively 'known about' the connected facts of consciousness." Fusion and association are the two types of the second mode of synthesis. Now it is difficult to see how a connection which is really conscious 'stuff' can be regarded objectively as a mere 'reflective' bond. This is surely 'atomism' in its most vicious form. The analogy with chemistry and physics, which are said to take their connections 'for granted,' is very misleading; both because (1) these sciences assume that no material (matter) appears when elements unite, and because (2) every combination of physical elements does call for an account of all the new characteristics and modes of reaction that issue when elements combine. Again, we may ask, what is the appropriateness of naming an external, 'reflective' connection of elements a 'fusion?' And how is one to 'fuse' elements in the cool process of reflection? The problem is made doubly serious by the manufacture of a vast number of new elements—intensities, extensities and relational feelings. Think of trying to weld, in the chilling flicker of meditation, "the feelings of redness, yellowness, colorless light, brightness, bigness, odor, coolness, pressure from joint and skin stimulation and pleasure" (158). The author partially redeems the situation by deserting her definition of fusion in writing of 'fusion degrees,' the closeness with which the diverse elements are connected. The closeness or looseness of a fusion is, certainly, a part of the experience, a direct mode of consciousness which does not wait upon an *ex post facto* inquiry. But if fusion and association are external connections, the distinguishing mark which separates the two types is doubly external: the connected factors are, in the one, peripherally excited, in the other, one factor, at least, is centrally excited. But whether the form of connection is

different in the two cases, or whether the factors connected are qualitatively different, in the two cases, we are not told. We must infer, therefore, that the chief types of synthesis are not necessarily different themselves, do not even synthesize different materials, but that they have different physiological antecedents. We cannot regard them, then, as psychological classes.

The author finds two attributive elements, pleasantness and unpleasantness. The elementary student would, perhaps, have been more instructed by a full discussion of the affections themselves and a briefer account of physiological theories of their origin.

The treatment of the relational elements is spoken of as "simply a developed and systematized statement of the teaching of James." There is also apparent in it, however, the strong influence of the traditional English School; Locke, Hume, Mill and Spencer. The reinstatement of Spencer's term, 'relational feeling' (*i. e.*, a feeling that relates other feelings), in place of 'transitive state'—James's very apt name for certain aspects of consciousness—clearly marks a return to the more traditional view of relations. The later literature on the subject, even the important contributions of the Austrian and German schools which take up the problem in a fresher and more interesting form, seems not to have been taken into consideration. One is really disappointed to find nothing new in the chapter. Analytic work in psychology is, without any question, difficult and there is a constant tendency to invoke 'the elements' for a miraculous multiplication of raw materials; but, although marvellous increase is reported, from time to time, in various quarters, there is usually good ground for skepticism when the discovery of a new lot of elements is announced. The enormous impulse toward analysis, which is characteristic of any new science, has, undoubtedly, overshadowed the problem of synthesis, but the problem cannot be successfully evaded by the introduction of raw relations. For the physiological substrate of the relational feelings, the author adds to Spencer's crude hypothesis the only less crude associationist conception of Flechsig and suggests, accordingly, connecting fibres and certain cells in the association-centers.

A striking and almost bizarre exhibit of relational elements is introduced in the chapter on attention, where the emphasis is laid on the element of 'clearness.' This is really an object-lesson in the intemperate use of relations. If intensity and bigness were to be considered as elements, it is not difficult to make oneself believe that clearness (an indubitable feature of attention) should, likewise, be considered as elemental; but it is not so easy to see why clearness should be made a 'relational' element while these others are not. What, one may pertinately ask, does it relate? To detect the fundamental fallacy involved here it is only necessary to contrast the account under consideration with, *e. g.*, Kuelpe's treatment of attention as a state of consciousness, in which contents undergo modification in various directions—clearness, duration, reproductivity;—a state and not a definite irreducible bit of consciousness.

Miss Calkins's illustrations of the fact that "we sometimes attend to experiences which are not affectively toned" (141) seem to have been unfortunately chosen. The "well-known phrase" is surely attended to, not because it is insignificant, but because it is 'well-known,' familiar and therefore, pleasant; likewise the "æsthetically indifferent face, if familiar" is attended to for a similar reason. I cannot agree that "many acquired interests are indifferent." They are, it may be granted, often less strongly toned than 'primary interests.' This is natural, because acquired interests mean, until acquirement is perfected, constant fluctuation and, hence, a low degree of attention. But

no one that insists on the invariable presence of affection in the state of attention would, I think, be so rash as to affirm that clearness is "identical with pleasantness or unpleasantness." Such an identification would, indeed, scarcely be considered except by a theory that makes attention an element-in-consciousness, possessing a single characteristic. When, on the other hand, attention is regarded as a state or condition which affects the whole range of consciousness, there is no incongruity in giving it half a dozen predicates.

The second part of Book I deals with "concrete mental experiences." It discusses, in their two-fold aspect of idea and of "relation of a self to other selves," fusion and association (to which reference has already been made), perception, imagination, memory, thought, recognition, emotion, volition, belief, will, faith and also certain "typical personal relations." The line is drawn somewhat too closely between this section and the first part of the book. Analysis is undertaken, in the author's opinion, merely for the scientific purpose of enumerating exhaustively the fundamental features of the psychic experience. She intimates, at the beginning of part II, that she is done with "this 'post mortem study' . . . . . of those 'artificial abstractions,' the structural elements of consciousness and will turn" to the consideration of "'entire conscious states as they are concretely given to us.'" This sharp distinction between analysis and the study of the idea is, however, partially erased later by the qualifying, if contradictory, statement that "scientific acquaintance with an idea . . . includes . . . . . analysis into structural elements" as well as explanation.

Miss Calkins's argument for a psychology of selves appeared before the publication of the *Introduction* (see *Philos. Review*, IX, 490). It distinguishes 'atomistic' psychology—"a structural science of contents of consciousness"—from the "science of conscious selves."

This dichotomy calls for two or three general remarks. (1) It is not evident, in the first place, that a structural and an atomistic psychology are identical, although it is historically true that the two have not been well separated in practice. A structural psychology that is no more than a diligent search for atomic units scarcely deserves the name of a science. It is only the initial chapter to a science. Mind, considered for its own sake, is surely more than mental atoms, and requires something more than an analytic method, important as this is. It is, by all means, unfair to the facts to classify, as 'atomic' all investigations into consciousness-as-such. 'Structure' implies tissue and organization rather than atoms. The fashion (or the passion, should one say?) for using biological analogies in everything not-biological may not subside for some time; but, meanwhile, do not let us obscure our terminology by blending chemical and biological figures. Again (2) the antithesis between the two main types of psychology is not entirely clear to the writer. Why should a study of the 'self,' any more than of any other of the uses to which mental processes are put, be set over against the investigation of the mind? It is true that the apprehension of the "I" or of the "thou" is of frequent occurrence in experience; but so, also, is apprehension of countless other things. The self, as an idea or a concept, is only one among many. Surely, a psychology of selves cannot take the place of an exhaustive examination of the multitudinous offices which contents-of-consciousness fulfill. The recognition of conscious individuals, of 'shared' experiences and of 'attitudes' is certainly an important feature of mind, or better, perhaps, of the psychophysical organism;—but it is, by no means, the only one. The psychology of selves cannot, then, be considered as antithetical, or even complementary, to an 'atomistic' account of

mind, for it reveals only one aspect of consciousness-in-the-mass, of what James has so ably treated as the "stream of consciousness." Neither can it hold a similar position with respect to a 'structural' psychology, for it, by no means, exhausts the 'functions' which are the inevitable correlates of mental existence. In the third place (3), it is necessary to draw a line of distinction between a science and the use of scientific method. No one, I imagine, will deny that the self can be treated by methods and appliances common to the sciences; but it is doubtful whether it furnishes material for a science. If such a science comes into existence, it will, undoubtedly, come as a social psychology.

Consciousness of self, we are told, can only be described—apart from analysis, at which, curiously enough, the author makes no attempt—as "consciousness of myself contrasted with other selves, and second, as consciousness of my varying relations or attitudes to these other selves." This fact shows the essentially "social nature of the self," and from this, again, follow its "two fundamental phases," the egoistic, imperious phase, and the altruistic, adoptive phase, in which the emphasis is, in the one case, on the 'myself' and, in the other, on the 'other self.'

Perception is treated both as "mere idea" and as a social (sharing) experience of selves. Percepts are analyzed into sensational and relational elements, their history is given, and they are classified as "pure" and "mixed." As experience of selves, the perception's essential characteristic is said to be its social, 'altruistic' character; perceptions are 'public property.' This point seems to be forced. Does perception, even in the completed form of "my perception of an object," necessarily involve the like experience of others? The sharing is, surely, something external to the perceiving—however much social experience has figured in the *history* of the notion of external reality (*cf.* J. Royce, *Philos. Review*, III, 513)—and, moreover, it is the *object* that is shared, not the act of perceiving; the latter is as individual as imagination and memory which may, indeed, from one point of view, be said to be shared. For one of the first aims of art and of history is to extend participation in just these experiences. Although they have not a common, 'external' permanence in the world of sense, their objects and their functions are as truly—as much and as little—shared as are perceptions.

The chapter on perception would have been more satisfactory if the previous accounts of fusion and association had been more adequate. It is curious that illusions (defined as perceptions which do not "directly correspond with any outer object:" an extremely loose and ambiguous definition, by the way) are often 'pure' in distinction from 'mixed' (184); although pure perception "can occur only in primitive or in half-unconscious states" (180). Finally, it may be asked how this classification of perceptions can be fundamental, if peripherally and centrally excited sensations, on which the distinction rests, are the same in kind, as the author intimates (186-7).

In the treatment of memory, Miss Calkins follows Sully in the use of the term 'reproductive imagination.' The chapter is brief, pedagogical and jejune. There is nothing in it to tell the student what memory is, in terms of mind. It is worth noting that the *Introduction* deals, for the most part, with memory and imagination as functions of mind—the very terms are explicitly functional terms,—and yet makes one a subclass under the other. One would rather expect insistence on their essentially diverse functions (construction of a definite past and elaboration of a separate world of 'reality') and insistence on their ultimate likeness as contents-of-consciousness.

The two-fold treatment of thought (generalization, judgment and reasoning) is clear and extremely well carried through. In the analytical part, one cannot, however, avoid the feeling that the relational elements of 'generality' and of 'wholeness' are entirely overdone, and that they do not really supply the lack so woefully felt in all associationistic accounts of judgment and reasoning. The social character of thinking appears in its universality. "No man appropriates the multiplication table" or the axioms of Euclid. "Our thoughts are never regarded as personal property." "We do always in our thinking assume the conceivable universality of the experience, we acknowledge that other selves have, or may have, the concepts that we possess." Here, again, there seems to be a confusion of the 'universals' (a logical term) that are reached in generalization and a sharing of thoughts in a community of individuals. The 'anyness' of the general notion surely does not refer to 'any-thinking-body,' but to any-one-of-a-class. It is neither personal, psychological nor social, but epistemological. The triangularity of "any triangle" is scarcely "the part experienced by anybody who thinks of the triangle" (220); but the common mark of all things in a certain class. The social covenant is a subordinate and unessential feature of the concept.

The chapter on Recognition (XIX) deals chiefly with the 'feeling of familiarity,' which is found to be a 'relational experience,' further analyzable into the 'relational feelings' of 'same' and of 'past,' the latter "probably involving a feeling of linkage or connection" (260). Why not commit oneself unreservedly to the 'psychologist's fallacy' and push analysis to the phonetic elements of the words 'same' and 'past?'

"An emotion is defined as any complex fact of consciousness, of which pleasantness or unpleasantness is an important feature" (263). The taste of lemonade or the feeling of hunger, then? Even Sully's loose classification is more rigid, for it limits emotions to "*mental* feelings." There are two main types of emotion, those of happiness and those of unhappiness. The "ideas of bodily change" form a "secondary constituent of emotions." On the 'self' side, "emotion is the relation of individual with individual." Considered thus, emotion is either personal or impersonal (*i. e.*, it involves an attitude toward selves or things).[1] Either the social nature of the self (152) is not fundamental or this classification transcends the limits of a 'self' psychology.

The fundamental dualism of method which we have been observing works best in the realm of will and faith, where the self is, without doubt, thrust well into the foreground. Will and faith as the "relations of self to selves" (299) correspond, respectively, to volition and belief in idea-psychology. The treatment of these subjects is particularly well thought out. The general point of view is much like that of James. If the reviewer were to make any general criticism, it would be that will and faith, as active relations of selves, are too sharply set off from the 'passive' attitudes of emotion.

The chapters on Comparative Psychology give a brief and general statement of the facts of the animal and the child mind. The author is less at home here, depending largely on a few secondary sources; nevertheless, she writes clearly and comprehensively. An occasional slip is to be noticed. The invertebrates are said to have "no olfactory or gusta-

[1] "To quote Titchener, in emotion, the perception or image 'is swamped in the affection.'" I cannot verify the quotation, although I find the phrase "swamped by affection" (*Primer*, 1899, 141). Both the *Primer* (*loc. cit.*) and the *Outline*, 1899, 229, say very explicitly that the ideational unit in the emotion is not the perception or image but the assimilation; that the emotion demands a situation.

tory organs" (357), although the end-organs of pressure, taste and smell are, we are told, developed before visual organs (357), and four types of eyes are found in invertebrates (361). In another place, the reader is gravely informed that fishes "have no brain, and therefore no olfactory lobe" (360). Again, certain statements are made broadly where fuller knowledge would have counselled caution; *e. g.*, "there is no doubt at all that the higher vertebrates and the insects possess sense-images, as well as sense-precepts" (365). And one can but look with suspicion upon a social consciousness that justifies itself by the animal heroisms of a Thompson-Seton.

The sections on Abnormal Psychology furnish only a brief outline, but the point of view set forth is safe and conservative. The chapter does not show a wide knowledge of the literature. The author has reserved discussion of various difficult problems for the appendix. This helps the perspective and the proportions of the book and will, doubtless, be an aid to the junior student.

The reviewer's appreciation of the book in hand may be set down much more quickly than his adverse criticisms; not because it is given less freely or heartily than they, but because, quite on the contrary, it embraces the work as a whole, while they have dealt, for the most part, with minor particulars. The book is exceedingly good to read. The ease and directness of the style win one from the first page. It is, however, the vividness of presentation and the evident reality of the experiences discussed that will best serve the author's specific purpose. This is exceedingly important in an elementary text-book in psychology. The volume shows real psychological living, and it will help the student who comprehends it to find and to use psychological material in his own experience. Nearly every new topic is introduced by a concrete example which will start associations and make the topic significant. There is, moreover, a wealth of illustrations, chosen, as a rule, with care and discretion. The subordination of bare schemata and classifications is, in the writer's opinion, exceedingly wise. The attractiveness of psychological thinking can never be set before the youthful student by definitions and rubrications, however clear and logical these may be.

At the same time it cannot be denied that the *Introduction* is difficult. It has, quite often, the psychological world, apparently, more than the student, in view. The discussion of some problems, *e. g.*, the problem of attention, presupposes a knowledge of the subject well beyond the grasp of the elementary student. This mode of treatment may, however, be intentional, since the author believes that a text-book is a "subsidiary adjunct" in teaching science.[1]

I. M. BENTLEY.

[1] The following minor points may be noted: Red, green, yellow and blue are said to be the elementary color qualities. Other colors are complex. "Yellow-green looks like yellow and green; that is, it is introspectively analyzable into yellow and green." But why not say, in the same way, that green looks like olive and peacock, and is therefore complex? Simple qualities can be similar and yet not be identical with each other. As a matter of fact, the author herself says: "green, for instance, appears to us like red and blue and yellow, but unlike sour and hot" (22). . . . . The following statements are ambiguous. "The re . . . . . terminating in the ciliary muscle." "The rods and cones, the only part of the eye on which the light can act directly" (24). "The ciliary muscle . . . . whose contraction . . . . enlarges the pupil" (24). With steady fixation straight ahead, a scarlet pencil when moved in "will seem gray or black until it is almost directly in front of my eye." . . . . Some of the corners of fig. 4 are rounded and some are not. . . . . The mucous membrane of the nasal cavity is said to be 'composed' of nerve fibres (63). . . . . For the two-point limen, "no more probable or adequate hypothesis has been proposed than Weber's unstimulated nerve fibres (69). . . . . Too great stress is laid upon relativ emperatures of the body and surroundings in the production of warm and cold s ns (77). *Cf.* Ebbinghaus. Psychologie, 345-6, for adaptation. . . . . The "s of consciousness of

Grundzüge der Psychologie, von HERMANN EBINGHAUS. Veit & Comp., Leipzig, 1902. pp. 321-694.

We have here at last the second half of the writer's psychology. Although avowedly but an introduction, it is copious. The writer first treats of the soul, its concepts, names, organ, seat, relations to the body, and consciousness and unconsciousness. In the second book, the structure and function of the nervous system is given about 70 pages. The third book treats the simplest psychic forms, beginning with the eye and passing to hearing and the lower senses. It also treats sensations in their general laws, space, time, unity, threshold, and finally and very briefly, concepts, feeling and will, comprising about 300 pages. The fourth book, about 120 pages, is devoted to the general laws of psychic life, such as attention, memory, association, reproduction, habit, etc.

The Mental Functions of the Brain. An investigation into their localization and their manifestation in health and disease. By BERNARD HOLLANDER. G. P. Putnam's Sons, New York, 1901. pp. 507.

On the basis of the clinical records of eight hundred cases of localized brain derangements, this Freiburg physician attempts to reinstate phrenology. After a survey of the present state of mental science, he discusses first the pathology of melancholy with reference to the seat of the lesion in cases of injury, tumors, inflammatory disease, hemorrhage, symmetrical atrophy, cranial abnormality and psychic blindness. In the next chapters mania is treated in its various forms with suspicion, persecution, kleptomania, etc. Localization for words, number, color blindness, the centers of exaltation, mimicry, sympathy, veneration, and finally the cerebellum, are discussed, together with the relations between brain and skull and the significance of cranial contours, the doctrine of free will, etc. The history of the discoveries of Gall, Spurzheim and Combe are treated, and the disrepute of phrenology described to ignorant professionals. The opposition to phrenology by Spencer, Bastian, Flechsig, Minot, and especially Comte is treated, and finally testimony as to the truth and usefulness of phrenology by a score of medical men is adduced. A concluding chapter sums up the evidences as showing that the frontal lobe is the seat of the intellect, the parietal of the emotions, the temporal of the propensities, and the occipital of the affections. The all pervasive error of the work is the neglect of negative cases where the specific organ is morbidly affected without injury to the faculty there located.

Relation of Psychology to Music, by E. F. BARTHOLOMEW. Rock Island, Ill., 1899. pp. 310.

These lectures, originally given in the Augustana Conservatory of

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the body's position (81) is a misleading phrase. . . . . On pp. 93-4 there is a confusion of clang-color and an alleged 'extensity' element in tones. A reference to Stumpf's treatment in the *Tonpsychologie* should clear up the matter. . . . . "It . . . . . is very improbable that any cerebral distinction results from the slight difference of the retinal images" (99). Then how is stereoscopy possible? . . . . . A bare statement of the "psycho-physic law" is given, without any reference to its significance. It [illegible] however, difficult to see just what such a law can mean when applied to heter[illegible]ous "intensity elements" which are different for different sensations (111). . . . . Wundt is made to call affection an 'attribute' (114), without any reference to his most recent position (*cf. Grundriss*) which makes the affections separate elements.

Kraft Ebing is written for Krafft Ebing, Théodor for Theodule Ribot, Hanssen for Hansen, Nicholls for Nichols, Thomas for Anstruther-Thomson, Z. Stern for L. Stern. Taste buds are not confined to the circumvallate papillæ (v 58), but are to be found both on the anterior surface of the tongue and on the epiglottis. . . . . "Other cells support the inner and outer 'organs of Corti'" (51). But the organ of Corti includes the rods of Co[illegible]i, the inner and outer hair cells and the *lamina reticularis* (Schaefer).

Music, are by a man not well known in psychological circles, but evidently a deep soul thinker, whose treatment of the subject is often new and original, and always highly stimulating and suggestive. Under the means of musical expression, he treats of the nervous system, the eye, the range of hearing and listening. Other interesting chapters are on habit, association, memory, imagination, feelings and emotions. Not a few of his illustrations are interesting and original. This work represents the closest point of contact yet made between modern psychology and musical theory.

Saint Anselme, par DOMET DE VORGES. F. Alcan, Paris, 1901. pp. 334.

After characterizing the civilization of the eleventh century, science at the beginning of the Middle Ages, and the pre-scholastic schools, Anselme's life is briefly treated in twenty pages. Each of his works is then roughly outlined, and his theories of knowledge, truth, human nature, the soul, liberty, God and his famous proof of his existence, are concisely presented.

The Doctrine of Space and Time, by GEORGE S. FULLERTON. Reprinted from the *Philosophical Review*, Vol. X, Nos. 2, 3, 4, 5 and 6.

Professor Fullerton is now surely our best American authority on the philosophy of time and space. He has wrought into a clear consistent whole the views of previous thinkers with abundant and luminous suggestions of his own.

Chapters on English Metre, by JOSEPH B. MAYOR. University Press, Cambridge, 1901. pp. 308.

The chapters are as follows: introduction; antiquarian A-priorism logical A-priorism; æsthetic intuitiveism; natural or A-posteriori system; metrical metamorphosis; two recent metrical systems; naming and classification of metres, illustrations from Tennyson; naming and classification of metres, illustrations from the hymn-book; blank verse of Surrey and Marlowe; Shakespeare's blank verse—Macbeth; Shakespeare's blank verse- Hamlet; Modern blank verse—Tennyson and Browning; Shelley's metre; the English hexameter.

The Man Christ Jesus. A Life of Christ, by W. J. DAWSON. Grant Richards, London, 1901. pp. 454.

The author describes his object as "to depict the human life of Jesus as it appeared to his contemporaries with the purposed negligence, so far as is possible, of the vexed problems of theology and metaphysics." It is necessary to perpetually rewrite the life of Christ, because every age must speak a new language of religion. It is the only life in which the world is permanently interested. Although he began to write solely from the view of Jesus' human efficiency, this plan seemed taken out of his hands as he went on. It certainly brings many things very visibly and objectively before us, but this in almost exact proportion to the fidelity with which the author adhered to his original and pragmatic plan.

Die Kindheitsgeschichte Jesus nach Lucas, von JOHANNES HILLMAN. Jahrbücher für Protestantische Theologie, Vol. XVII, No. 2, pp. 192-257. Braunschweig, 1891.

This is the fullest digest of the whole story of the childhood of Jesus according to Luke, who tells it most fully, with an appendix on the way in which Luke treats the theme.

Die Analyse der Empfindungen nnd das Verhältniss des Physischen zum Psychischen, von E. MACH. Gustav Fischer, Jena, 1902. pp. 286.

It must have been twenty years since the author published the first pamphlet bearing this name. The third edition is much enlarged, both by additional topics and by supplements to those treated formerly. It is indeed such a standard and now so complete, that it is high time that it was done into good English.

Essai de psychologie militaire individuelle et collective, par M. CAMPEANO. Georges Fanchon, Paris, 1902. pp. 214.

After a preface by Ribot and a long premise on general and individual psychology, the will, attention, etc., the author proceeds to characterize the modern soldier; his relations to the army; the qualities of generals; the difference between infantry, cavalry and artillery; and finally discourses on the care of health in the service and military justice.

Experimental Sociology. Descriptive and Analytical. By FRANCES A. KELLOR. The Macmillan Co., New York, 1901. pp. 316.

This very helpful book describes—Elements of experimental criminal sociology; temporary laboratories and criminal characteristics; anthropometrical measurements; psychological tests; sociological data; suggestions for laboratories and child study; environment and criminality; the increase in criminality of women; penal and correctional institutions in the North; penal system in the South; defects in penal and correctional systems; relation of criminal sociology and criminal jurisprudence; suggestions for prevention of criminality.

The Nature and Development of Animal Intelligence, by WESLEY MILLS. T. Fisher Unwin, London, 1898. pp. 307.

The author has long been favorably known for his admirable studies on several species of young animals, but his papers have been widely scattered and inaccessible. This is by no means entirely a reprint, but while containing the best of his special studies it presents us his own wider view, and tells us how to study animal intelligence; the problems of comparative psychology; the functional development of the cerebral cortex in different groups of animals; somatic correlations with psychic development with special reference to the brain; and a final and interesting discussion on instinct.

Intuitive Suggestion. A new theory of the evolution of Mind. By J. W. THOMAS. Longmans, Green and Co., London, 1901. pp. 160.

Convinced that there are more missing links in the evolution of the mind than in that of the body, as well as great difference of opinion whether certain faculties could result from natural selection at all, this writer brings forth a new name, if not a new theory. He describes what he terms intuitive function in organic matter, then in the world of organization and life; treats of the relationship between the intuitive faculty and nerve action or mind; traces the development of the senses and the subsequent formation of the mind, the moral sense, and finally the intuitive functions as indicators of the coming time.

Neurological Technique, by IRVING HARDESTY. University Press, Chicago, 1902. pp. 183.

This work is designed as an introduction and laboratory guide to the study of the nervous system. It discusses the need and action of reagents; various methods like those of Nissl, Bethe, Apathy, Kupfer, Golgi, Weigert, Marchi and others; with special sections on the mode of treating embryos and a laboratory outline for dissection.

Capillaranalyse Beruhend auf Capillaritäts-und Adsorptionserscheinungen mit dem Schlusskapitel: das Emporsteigen der Farbstoffe in den Pflanzen, von FRIEDRICH GOPPELSROEDER. E. Birkhauser, Basel, 1901. pp. 545.

This volume with its tables, cuts and colored charts seems to be the most exhaustive study of the phenomena of capillarity absorption and the way and rate in which coloring material ascends in plants, yet made. Lists of colors of different substances with their action in different species of plants are almost ideally full and complete. We are unable to discuss it in detail, but it is a model of scientific research which also has a wide field of practical application.

An X-Ray and Dissection of the Ureter and Utero-Ovarian Artery, by BYRON ROBINSON. Chicago.

A seven page pamphlet explains the utero-ovarian vascular circle, which was carefully dissected and photographed in two large charts which appear to be very carefully drawn, one of which is colored.

Altschwäbische Liebesbriefe, von ALBERT RITTER. Graz, 1898. pp. 118.

Das Geschlechtsleben der Heiligen, von F. STEINGIESSER. H. WALTHER, Berlin, 1902. pp. 64.

Das Geschlechtsleben des Weibes, von ANNA FISCHER-DÜCKELMANN. Hugo Bermühler, Berlin, 1902. pp. 219.

Experiments on the Effect of Muscular Work upon the Digestibility of Food and the Metabolism of Nitrogen conducted at the University of Tennessee 1897-99, by CHARLES E. WAIT. Govt. Print, Washington, 1901. pp. 77.

Thesis in Ontology or the Philosophy of Entity, by ARTHUR L. FROTHINGHAM. (2 parts, pp. 34.) Princeton, N. J.

We have perhaps in this country no one more devoted to ontology than the author of these pamphlets. From time to time he prints brochures at his own expense and sends them to those who are interested in these subjects for criticism and suggestion. While his terminology is somewhat difficult, it is plain that the author is very much at home in the literature, especially that of the mediæval period; and while perhaps no two thinkers in this field agree, all who ponder his questions will not only be interested, but stimulated.

The Theory of Prosperity, by SIMON N. PATTEN. The Macmillan Co., New York, 1902, pp. 237.

The first part discusses income as determined by existing conditions, including work and pay, monopoly, advantage, and investments. The second part treats of income as determined by heredity or more specifically as fixed by struggle; as increased by adjustment; as modified by economic rights, whether those of leisure, society, of the market, or exceptional rights.

Arthur Schopenhauer. Seine Persönlichkeit, seine Lehre, sein Glaube, von JOHANNES VOLKELT. E. Hauff, Stuttgart, 1900. pp. 392.

We have here an excellent picture of Schopenhauer—the man, his life, his work. The first is briefly told in twenty pages, and his personality is characterized in twenty more; while to the motives that prompted him to his conclusions, his method, his theory of knowledge, his position in philosophy, his metaphysics, characterizations of the world will and its objectivization, man as intellect and will, pessimism, genius, æsthetics, morals, pity, renunciation, significance of the individual and of Schopenhauer himself, a chapter is given. Thirty pages of suggestive notes and a chronology of the author are appended.